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AN EXPERIMENTAL STUDY OF THORNDIKE'S THEORY OF LEARNING*

From the Psychological Laboratory of the University of Wisconsin

MARGERY HAYDEN TROWBRIDGE AND HULSEY CASON

INTRODUCTION

The experiment described in the present paper was designed to test the validity of some of the conclusions which Thorndike drew from a preliminary study of improvement in line-drawing. The study with which we are principally concerned was Experiment B of the two experiments which Thorndike described in the *American Journal of Psychology* in 1927 (3).[1] Thorndike was responsible for the general plan and treatment of the results of the above study, but Bregman and an unmentioned number of assistants working under her conducted all of the tests and training and supervised the scoring and tabulating of the results. The general plan of the experiment described by Thorndike was as follows.

> "The *S* was seated, blindfolded, at a table opposite the *E*, and in front of a drawing-board, along the left-hand edge of which a strip of veneer, about 2 in. wide, had been fastened in such a way that a large sheet of cross-section paper (16 x 21 in.) could be slipped between it and the board and fastened to the board by means of two or three carpet tacks. The strip of veneer served as a fixed starting edge for all lines. The cross-section paper itself was so ruled, in pencil, as to make it possible for the experimenter to tell readily the length of any line drawn from the strip as a zero point. The *S* was instructed to draw lines of a given length, starting always from the strip of veneer at the left, and to wait after each line until hearing the score called, before drawing the next line. He was required to draw each line with one continuous, quick movement."

S was instructed to draw a 3-, 4-, 5-, or 6-inch line, and E said "Right" when S's line was within 1/8 inch of the correct length in

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[1] See also (4, pp. 30-81).

the case of 3-inch lines, or 1/4 inch of the correct length in the case of 4-, 5-, and 6-inch lines. E said "Wrong" in all other cases. In a control test, E did not say anything after S drew the line, regardless of its length. The results were not as reliable as might have been desired, but they seemed to show that E's saying "right" and "wrong" in the manner described above improved S's efficiency in the line-drawing function.

This experiment was devised by Thorndike to test the proposition "that what comes after a connection acts upon it to alter its strength." Thorndike said this was a statement of the "law of effect." He claimed that the above experiment was "a crucial test of whether the after-effects of a connection. . . . strengthen or weaken it." It was thought that E's speaking the words "right" and "wrong" produced the "effects" of satisfyingness and annoyingness in the S, and that these "effects" were a "fundamental, general, and direct favoring of the successful connections."

Thorndike expressed the opinion that the improvement in line-drawing was not the result of repeating some "inner counterpart" of the line-drawing act. He minimized the S's use of cues and claimed that there were two reasons for emphasizing the direct favoring of the successful "connections."

> "First, we have no evidence that a person improves any faster by trying deliberately to retain or recall such cues than by simply drawing line after line as he feels like doing. Second, it seems unlikely that cats, dogs and rats carry on any such deliberate reviews or rehearsals in the bulk of their learning."

This argument seems to be somewhat confused,[2] and its relation to the present topic is not clear; but Thorndike expressed the opinion that an extension of his experiment would strengthen the case for effect. In the present paper we are principally concerned with an experimental study of the conditions under which improvement occurs in the line-drawing function, and the present study is an extension and modification of Thorndike's experiment.

METHOD USED IN THE PRESENT EXPERIMENT

The *general* method used in the present experiment was similar in several respects to that employed by Thorndike. S was blindfolded during the experimental period and could not see any of the apparatus or material that was used. A drawing-board was on a table, and E and S sat at the opposite sides of this table, S sitting squarely in front of the table. In the starting position before drawing each line, S's two hands were directly in front of him, 12 inches from the near edge of the table, and 3 inches apart. S's left hand grasped a stationary peg between the thumb and first two fingers, and his left hand and arm remained in a stationary position at all times during the experiment. The general procedure in the experiment was definitely improved by having S keep his left hand in a constant position with respect to his right hand. S held a pencil naturally in his right hand, and the starting position of the pencil was 3 inches to the right of the stationary peg. In drawing each line S raised his right hand and arm so that they would just clear the surface of the table, and he drew the line directly to the right with one continuous, quick, but fairly natural and easy, movement of the whole arm and hand. In the present experiment the S's drew only 3-inch lines, since there seemed to be a disadvantage in having them draw lines of different lengths.

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[2] The theoretical questions involved in the pleasure-pain theory have been discussed by Cason in a separate article (2).

The *drawing-board* was 12 inches long, 8 inches wide, and 3/8 inch high. The board was fixed to the table at the left end by a vertical pivot, and E could move the right end nearer the S or farther from him. If, in spite of instructions to the contrary, S persisted in drawing lines that were not exactly parallel to the side of the table, the distance between the S and the right end of the drawing-board was changed slightly by E so that S's lines were at least parallel to the side of the drawing-board. The board remained quite stationary after S placed it in position. A strip of wood, 8 inches long, 1 1/2 inches wide, and 3/4 inch high, was placed crosswise on the drawing-board near the left end, and parallel to, and just 9 inches from, the right edge of the board. The left-hand portion of this wooden strip was hinged at the far and near ends in such a way that the right edge of the strip could be raised by E, but it was ordinarily pressed down against the surface of the drawing-board by the tension of a strong rubber band. E placed the ruled paper, on which S was to draw the lines, on the drawing-board with the left edge of the paper under the right edge of the wooden strip. The right edge of the hinged wooden strip served as a clamp for the paper and held it in place. The starting-point in drawing each line was the right edge of the wooden strip. The paper on the drawing-board was ruled crosswise in lines 1/8 inch apart and in heavier lines 1 inch apart, so that E could read the length of each line drawn by S and record the score promptly.

The relative positions of the S, the E, the table, and the drawing-board were the same for all S's, and the instructions for drawing the 3-inch line were also always the same. *Four different procedures* were used, however, and in three of these procedures E gave S a stimulus of some kind after each line was drawn. The four different procedures were as follows.

1. *Blank.* E did not say anything after S drew the line. This method was used as a control procedure.
2. *Nonsense.* E spoke a different nonsense syllable when S finished drawing each line. A list of 100 nonsense syllables was used, and a different syllable was spoken after each line.[3] This procedure was designed to check the influence of distraction, or possibly an increase in the S's attention, on the line-drawing function.
3. *Right-wrong.* E said "Right" as soon as possible after S drew the line if the line was within 1/8 inch of a 3-inch line. E said "Wrong" in all other cases. This procedure was essentially the same as that which was used in the experiment described by Thorndike.
4. *Correct.* E told S the length of the line he had drawn as soon as possible after he had finished drawing it. E said "Plus 2" if the line drawn was 2/8 inch longer than 3 inches, "Minus 7" if the line drawn was 7/8 inch shorter than 3 inches, etc. In this procedure S was simply told what he had done. It is well known that inner speech commonly aids in the performance of many different kinds of skilled movements of the hands and arms, and it seemed natural to assume that the efficiency of the line-drawing function would be increased if the S knew what he was and had been doing.

S drew 100 lines with one of the four procedures described above, and after a rest of five minutes he drew 100 more lines with one of the other three procedures. The four procedures were rotated in such a way that each procedure preceded each of the other procedures and followed each of the other procedures the same number of times. Each of the 12 different permutations of two procedures each was used two or three times with each of the S's of each sex.

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[3] The syllables were taken from a list which Cason had used in a previous experiment (1, p. 201).

It seemed that it would be interesting to study the effect of speaking a nonsense syllable on the line-drawing function, and to compare the S's efficiency when he was distracted in the "nonsense" procedure with his efficiency in the "blank" procedure. Thorndike claimed that most of his S's improvement in line-drawing in the R-W procedure occurred because of the satisfying and annoying effects of E's speaking the words "right" and "wrong." It therefore appeared especially desirable to compare the methods of improving the efficiency of this function in the "R-W" and "correct" procedures. In the present experiment this included a comparison of the S's introspections on the different psychological activities that were present and active under the different conditions, together with a study of the several different kinds of cues which were used by the S's in trying to draw lines three inches long. We thought that an analysis of the various methods used by the S's and a study of the psychological activities that were present in the four different procedures, as well as a comparison of the objective results obtained under the four different conditions, would afford the desired information in regard to the principal factors involved in improvement of the line-drawing function.

Margery Hayden Trowbridge was the E for all of the S's, and the method followed with each S included the following steps.

1. The S was blindfolded and was not allowed to see any of the apparatus and materials used in the experiment.

2. S was given detailed verbal instructions in regard to the position of his body and the position of his arms and hands. In each trial S placed the point of the pencil against the right edge of the wooden strip which was on the left end of the drawing-board, his right hand and arm were raised just above the surface of the drawing-board, and the line was drawn directly to the right with one continuous, quick, and natural movement of the whole arm and hand. Between trials, S's right hand and arm rested on the drawing-board. S found it convenient to keep the point of the pencil in the approximate position for beginning to draw the next line. The clicking sound of a telegraph relay was used as the signal for S to draw each line. The relay was in circuit with two dry-cell batteries, and E operated the relay by means of a mercury key. After the experiment began, the only words spoken by E were those which were required by the procedure being used.

3. S drew 100 lines with one of the four procedures described above, and E recorded the length of each line drawn.

4. During the 5-minute rest period which followed the drawing of the first 100 lines E engaged S in conversation about ordinary events of common interest, but the experiment was not discussed.

5. S drew a second 100 lines with a different procedure from that used with the first 100 lines.

6. After the second 100 lines, but before removing the blindfold, E asked S a number of questions from a typewritten form, the purpose of the questions being to determine the methods which S had used in drawing the lines, the factors which he thought were involved in the improvement of the line-drawing function, and his general insight into the purpose and nature of the experiment. None of the S's were aware of the general problem being studied in the experiment.

Subjects. A total of 60 S's was used, and there was an equal number of the two sexes. All of the S's were students at the University of Wisconsin.

RESULTS

Two different procedures were used with each S, and all 12 permutations of two procedures each were used with different S's. Results have been calculated for each procedure when it was used in the first position, and also for each procedure when it was used in the second position after each of the three other procedures. The results for each procedure when it was used in the first position are shown in Tables 1, 2, and 3. It may be recalled that when the line was drawn within 1/8 inch of a 3-inch line it was considered a correct response.

TABLE 1

AVERAGE PERCENTAGE OF CORRECT RESPONSES FOR EACH PROCEDURE WHEN THE PROCEDURE WAS USED IN THE FIRST POSITION

The score for each procedure was based on the results obtained with 15 S's and 1500 trials.

Procedure	Average percentage
Blank	13.6
Nonsense	5.1
R-W	22.6
Correct	54.8

Table 1 shows the average percentage of correct responses for each of the four procedures when the procedure was used in the first position. The scores for the different procedures were, for the "blank" procedure, 13.6; for the "nonsense" procedure, 5.1; for the "R-W" procedure, 22.6; and for the "correct" procedure, 54.8.

The introspections of the S's showed that in the "nonsense" procedure many of them were distracted when E spoke a nonsense syllable after each line. Several of the S's thought they would later be asked to recall the syllables, and other S's thought the syllables were intended as cues and they tried to interpret them as such. The E's speaking a nonsense syllable after each line was drawn lowered the efficiency of the line-drawing function from 13.6, the score for the "blank" procedure, to 5.1. This result is particuarly interesting in view of Thorndike's claim for the effects of the E's speaking the words "right" and "wrong" after S drew each line. The relative scores obtained with the "blank" and "nonsense" procedures show that almost any kind of stimulus may have some measurable *effect* on the results.

(At the beginning of the experimental period, practically all of the S's drew lines that were much shorter than 3 inches, and if they were ever to draw lines which approximated 3 inches it was necessary for them to know something about what they were doing. In the "R-W" procedure, the S's attempted to find the correct length by the trial-and-error method. In the "nonsense," "blank," and "R-W" procedures, they frequently drew a large number of lines without having any accurate information about the length of the lines they were drawing. (In the "correct" procedure, however, the S was told the length of each line just after he had drawn it. The difference between the "R-W" and "correct" procedures was simply a difference in the amount of aid given the S in the "correct" procedure in establishing in his own sensori-neuro-muscular system some kind of standard of a 3-inch line which he would try to approximate on later occasions.)

The situation in the "R-W" procedure is analogous to that of a person who is told to press his hand against a bar with force equal, for example, to 1/39 of an ounce. He might conceivably press with a force greater than 1/10 of an ounce on each of 100 successive trials because he has no accurate idea of the amount of pressure he is exerting. If the S is not experimentally inclined he might continue to press with a force greater than 1/10 of an ounce for 1000 trials, principally because he does not know, and has no way of knowing, what he is doing. In a situation of this kind, any information that E gives the S will naturally tend to improve the accuracy of S's performance. The difference between the "R-W" procedure and the "correct" procedure was simply a difference in the amount of information which S received in regard to a function in which no one could be expected to be particularly proficient without previous instruction.

The answers which the S's gave to questions asked them at the end of the experimental period showed quite clearly that they used several different kinds of cues. Cues were used by all of the S's except those who were given no information in regard to what they were doing and three S's who did not draw one line of the correct length in the "R-W" procedure. Forty-seven out of the 60 S's used cues in one way or another. At the end of the experimental period, all of the S's were asked, "When you had drawn a line of the correct length, did you use any kind of cues in trying to draw a line of the same length again?" All of the 47 S's who used cues understood what was meant by the question, and they volunteered explanations of the various methods which they had employed. The principal cues used were kinaesthetic cues from the arm and hand. A "timing" method was also frequently employed in which the S's estimated the time required for the whole movement of the arm. Of the 47 S's who used cues, 9 listened to the sound of their pencil as they moved it across the paper, and 7 counted at a regular rate and tried to determine the number of counts which they made while they drew a line of the correct length. At the beginning of the experiment, 37 S's attempted to estimate the 3-inch line by thinking of a ruler, by thinking of an inch and then taking it three times, or by estimating the width of the hand or the length of the thumb, etc. In the "blank" and "nonsense" procedures, a number of S's attempted to use visual imagery, but visual imagery in itself appeared to be of little assistance. However, this may not mean a great deal since nothing was of any great assistance in the "blank" and "nonsense" procedures.

It was clear from the answers which the S's gave that they *verbalized* extensively while they were trying to improve in the accuracy of the line-drawing. The motor act of drawing a line was by no means the only psychological activity that was taking place, and the act of drawing a line was also not the only objective and physiologi-

TABLE 2

AVERAGE ERROR IN UNITS OF 1/8 INCH FOR EACH PROCEDURE WHEN THE PROCEDURE WAS USED IN THE FIRST POSITION

The scores for each procedure are based on the results obtained with 15 S's and 1500 trials.

Procedure	Successive groups of 10 trials 1	2	3	4	5	6	7	8	9	10	Av. of first 3 groups	Av. of middle 4 groups	Av. of last 3 groups	Av. of all 10 groups
Blank	6.4	5.7	5.8	6.6	6.8	7.2	6.8	6.5	6.5	6.2	5.97	6.85	6.40	6.45
Nonsense	9.3	8.9	8.4	8.4	8.3	8.3	9.3	8.2	8.7	8.4	8.87	8.57	8.43	8.62
R-W	8.5	7.0	6.5	6.3	5.5	4.9	4.7	4.4	3.5	3.8	7.33	5.35	3.90	5.51
Correct	3.2	1.4	1.2	1.1	1.1	1.3	1.0	1.0	1.1	0.8	1.93	1.12	.97	1.32

cal activity that was present. In the analysis of improvement in the line-drawing function, the objective and introspective facts certainly do not justify sole reliance upon the satisfying and annoying effects of the E's speaking the words "right" and "wrong." Thorndike speaks in several places as if the only alternative explanation to his own is an explanation in terms of the ideas and images of possibly dogs, cats, and rats. It is clear from our results that this is not the case, and it also appears that our S's identified the cues they were using in a fairly satisfactory manner.

Some further objective evidence which directly or indirectly favors the above conclusions is given in Table 2, which shows the average error in units of 1/8 inch in each procedure when the procedure was used in the first position. In the case of each S, a calculation was made of the average error for each of the successive groups of 10 trials each. In the table, the figure 6.4, for example, is the average in the "blank" procedure of the first 10 trials for each of 15 S's, or an average of 150 trials. Similar averages are shown for each of the successive groups of 10 trials each for each of the four procedures. In the right-hand column of the table are given the averages in each procedure for all 10 groups. The relative efficiency of the four different procedures is the same when the scores are in terms of the average error as when they are in terms of the average percentage of correct responses (Table 1).

The fourth, third, and second columns from the right show some interesting details on the efficiency of line-drawing for the first three groups, the middle four groups, and the last three groups, of 10 trials each. In the "blank" procedure, the average errors of the first three groups, the middle four groups, and the last three groups were 5.97, 6.85, and 6.40, respectively, showing that the efficiency was highest for the first three groups and lowest for the middle four groups. In the "nonsense" procedure, the efficiency seemed to increase slightly during the 100-trial series, the three averages being 8.87, 8.57, and 8.43. In the "R-W" procedure, the efficiency increased quite rapidly during the series of 100 trials. In the "correct" procedure, most of the rapid improvement in efficiency occurred during the first 20 trials. The average error was larger for the last 30 trials with the "R-W" procedure (3.90) than for the first 30 trials with the "correct" procedure (1.93); and it was smaller for the first 10 trials with the "correct" procedure (3.2) than for next to the last 10 trials (3.5) or the last 10 trials (3.8) with the "R-W" procedure.

We have seen that the difference between the objective results obtained with the "blank" procedure and the "nonsense" procedure shows that saying almost anything may have some effect on the scores. The difference between the results obtained with the "R-W" procedure and the "correct" procedure also shows that the scores depend upon the nature of the information that the S receives. In the "correct" procedure, the S was told what he had just done, and this gave him some information in regard to the kind of changes that should be made for the next trial. The scores for the "correct" procedure contrast sharply with those for the "R-W" procedure where the S had to proceed somewhat blindly at first and where he was frequently given no information at all in regard to what he was doing until after a large number of trials had been made.

Table 3 shows the error in units of ⅛ inch for each procedure when the procedure was used in the first position, and this table gives more detailed information in regard to the central tendencies, variabilities, and reliabilities of the scores obtained with the four different procedures. The value of *N* is 1500 for each of the four procedures, and the figures in the table are quite reliable. The median error is 3.71 with the "R-W" procedure and 0.00 with the "correct" procedure, and it is especially interesting to note that the *S.D.* is largest with the "R-W" procedure and smallest with the "correct" procedure. The relative sizes of the errors with the four different procedures show conclusively that improvement in the line-drawing function depends upon other factors besides the satisfying

TABLE 3

Error in Units of ⅛ Inch for Each Procedure When the Procedure Was Used in the First Position

The scores for each procedure are based on the results obtained with 15 S's.

Procedure	Blank	Nonsense	R-W	Correct
N	1500	1500	1500	1500
$Av. \pm P.E._{Av.}$	6.45 ± .08	8.62 ± .08	5.51 ± .09	1.32 ± .02
$S.D.$	4.63	4.65	5.13	1.61
Q_1	2.5	5.1	1.2	0.0
$Q_2 \pm P.E._{Mdn.}$	5.69 ± .10	8.20 ± .10	3.71 ± .11	0.00 ± .03
Q_3	10.1	12.7	8.8	2.2

TABLE 4

Average Percentage of Correct Responses for Procedures Used in the Second Position

Each score was based on the results obtained with five S's and 500 trials.

		Second procedure		
First procedure	Blank	Nonsense	R-W	Correct
Blank	—	2.2	21.6	65.6
Nonsense	10.0	—	36.4	53.8
R-W	36.4	10.8	—	47.8
Correct	53.8	26.6	47.8	—

and annoying *effects* of the E's speaking the words "right" and "wrong."

Table 4 shows the average percentages of correct responses for the different procedures used in the second position after each of the other procedures. It may be recalled that for each S 100 trials were made with one procedure, and, after a rest of five minutes, there were 100 trials with one of the three other procedures. In the table, the figure 10.0, for example, is the average percentage of correct responses for the "blank" procedure when this procedure was used in the second position and when the "nonsense" procedure was used in the first postion. The figure 2.2 is the average percentage of correct responses for the "nonsense" procedure when this procedure was used in the second position and when the "blank" procedure was used in the first postion.

TABLE 5

Average and Median Errors in Units of ⅛ Inch for Procedures Used in the Second Position

Each score was based on the results obtained with five S's and 500 trials. In each pair the upper figure is the average and the lower figure is the median.

		Second procedure		
First procedure	Blank	Nonsense	R-W	Correct
Blank	—	8.8	5.7	0.9
	—	8.6	3.7	0.0
Nonsense	10.4	—	3.6	1.3
	12.1	—	2.2	0.0
R-W	4.8	10.1	—	1.2
	3.7	11.6	—	0.0
Correct	2.9	3.3	1.7	—
	2.4	2.7	1.6	—

Table 5 shows the average and median errors in units of ⅛ inch for the different procedures used in the second position after each of the other procedures. The figure 10.4, for example, is the average error in units of ⅛ inch for the "blank" procedure when this procedure was used in the second position and when the "nonsense" procedure was used in the first position. The figure 12.1 is the median error for the "blank" procedure when this procedure was used in the second position and when the "nonsense" procedure was used in the first position.

Tables 4 and 5 show that the "blank" procedure is, on the whole, superior to the "nonsense" procedure, and that the "correct" procedure is markedly superior to the "R-W" procedure. These results also show that both the "blank" procedure and the "nonsense" procedure were more efficient when they followed the "correct" procedure than when they followed the "R-W" procedure.

GENERAL SUMMARY

The S was blindfolded and was seated at a table, with his two hands directly in front of him and 3 inches apart. The left hand grasped a peg at all times during the experiment and this hand remained in a stationary position. When E gave the signal, S attempted to draw a 3-inch line directly to the right with his right hand. Each S drew 100 lines with each of two procedures; and the four different procedures that were used with different S's were as follows. (1) *"Blank"* procedure—E did not say anything after S attempted to draw a line 3 inches long. (2) *"Nonsense"* procedure—E spoke a different nonsense syllable after S drew each line. (3) *"Right-Wrong"* procedure. E said "Right" if the S's line was within ⅛ inch of a 3-inch line. In all other cases E said "Wrong." (4) *"Correct"* procedure. E told S the length of the line he had drawn just after he finished drawing it.

(The order of efficiency for the four different procedures was "correct," "right-wrong," "blank," and "nonsense," and the superiority of the "correct" procedure over all of the others was quite marked.) In the "nonsense" procedure, many of the S's were distracted by E's speaking a nonsense syllable after each line, and the results obtained with this procedure showed that saying almost anything may have a measurable effect on the efficiency of the line-drawing function.

At the beginning of the experimental period practically all of the

S's drew lines that were much shorter than 3 inches; and in the "nonsense," "blank," and "right-wrong" procedures the S's frequently did not know what they were doing a large proportion of the time. The principal difference between these three procedures and the "correct" procedure was that in the "correct" procedure the S's were told the length of the line they had just drawn, and they were therefore in a position to make the appropriate changes and adjustments on *later* trials. Nothing could be done about the *preceding* trial which was now entirely a matter of the past.

Four-fifths of the S's used cues of one sort or another in attempting to draw lines 3 inches long. The principal cues used were kinaesthetic cues from the arm and hand. A "timing" method was also employed in which the S's estimated the time required for the whole movement of the arm and hand. The S's verbalized extensively while they were trying to improve in the line-drawing, and their verbalizations had a direct influence on the efficiency of their performance. The introspections of the S's and the objective results that were obtained seem to show quite conclusively that improvement in the line-drawing function is not particularly dependent upon the satisfying and annoying "effects" of E's speaking the words "right" and "wrong." Improvement in this and similar activities is principally conditioned by the kind and the amount of information which the S receives in regard to a function in which no one could be expected to be particularly proficient without previous instruction.

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UNE ÉTUDE EXPÉRIMENTALE DE LA THÉORIE D'APPRENTISSAGE DE THORNDIKE

(Résumé)

Quand E a fait le signal, S, les yeux bandés, a essayé de dessiner une ligne de trois pouces directement à la droite avec sa main droite. Chaque S (sujet) a dessiné 100 lignes avec chacun de deux processus; et les quatre processus différents employés avec les différents S ont été les suivants: (1) Processus *blanc.* E n'a rien dit après que S a essayé de dessiner une ligne de trois pouces. (2) Processus *non-sens.* E a prononcé une autre syllabe non-sens après que S a dessiné chaque ligne. (3) Processus *correct-incorrect.* E a dit "Correct" si la ligne de S n'a pas été en longueur d'un huitème d'un pouce plus ou moins de trois pouces. Dans tous les autres cas E a dit "Incorrect." (4) Processus *correct.* E a dit à S la longueur de la ligne dessinée immédiatement après que celui-ci a fini de la dessiner.

L'ordre de l'efficience pour les quatre processus différents a été Correct, Correct-Incorrect, Blanc, et Non-Sens, et la supériorité du processus Correct sur tous les autres a été tout-à-fait marquée. Les résultats obtenus avec le processus Non-Sens ont montré que dire n'importe quoi peut avoir un effet mesurable sur l'efficience de la fonction de dessiner des lignes. Dans les processus Non-Sens, Blanc, et Correct-Incorrect les S n'ont pas su fréquemment ce qu'ils ont fait la plupart du temps. Dans le processus Correct les S ont su faire les changements et les adjustements appropriés dans les épreuves postérieures. Quatre quinzième des S ont employé des suggestions de diverses sortes, et les suggestions principales employées ont été des kinesthésiques du bras et de la main. On a employé aussi une méthode de règlement du temps où les S ont estimé le temps nécessaire pour le mouvement entier du bras et de la main. Les S ont verbalisé beaucoup en essayant de faire des progrès, et leurs varbalisations ont eu une influence directe sur l'efficacité de leur exécution.

Les introspections des S et les résultats objectifs semblent montrer assez conclusivement que les progrès dans la fonction de dessiner des lignes ne sont pas très dépendants des effets satisfaisants et ennuyants de la prononciation des mots "Correct" et "Incorrect" par E. Les progrès dans cette activité et dans les semblables sont principalement conditionnés par la nature et la quantité des renseignements reçus par le S.

Trowbridge et Cason

EINE EXPERIMENTELLE UNTERSUCHUNG DER THORNDIKE'SCHEN THEORIE DES LERNENS

(Referat)

Nachdem der Versuchsleiter das Signal gegeben hatte, versuchte die Versuchsperson, eine drei-Zoll-lange Linie gerade rechts mit der rechten Hand zu zeichnen. Jede VP zeichnete mit jedem von zwei Versuchsverfahren 100 Linien; und die vier verschiedenen Verfahren die an verscheidenen VPP verwendet wurden waren folgende: (1) *"Lehres"* Verfahren (blank procedure). Der VL sagte gar nichts, nachdem die VP versucht hatte, eine 3-Zoll-lange Linie zu zeichnen. (2) *"Sinnloses"* (nonsense) Verfahren. Nach der Zeichnung jeder Linie sprach der VL eine andere sinnlose Silbe aus. (3) *"Richtig-falsches"* (right-wrong) Verfahren.

Der VL sagt "Richtig!" wenn die Linie der VP sich innerhalb eines ⅛ Zolls einer 3-Zoll-langen Linie annäherte. In allen anderen Fällen sagte der VL "Falsch!" (4) *"Richtiges"* (correct) Verfahren. So wie die VP die Linie gezeichnet hatte, sagte ihr der VL sofort, wie lang die gezeichnete Linie wirklich war. Das wirksamste Verfahren war das "richtige." Dann folgten, in der Anordnung der abnehmenden Wirksamkeit, das "richtig-falsche," das "lehre," und das "sinnlose." Die Überlegenheit des "richtigen" Verfahrens über alle anderen war eine ziemlich stark ausgeprägte. Die mit dem "sinnlosen" Verfahren erzielten Resultate erwiesen, dass fast irgend Etwas eine messbare Einwirkung auf die Fähigkeit zum Linienzeichnen haben kann. Mit dem "sinnlosen," dem "lehren," und dem "richtig-falschen" Verfahren wussten die VPP oft während eines grossen Bruchstücks der Zeit nicht, was sie eigentlich taten. Mit dem "richtigen" Verfahren waren die VPP im Stande, in den späteren Versuchen die passenden Änderungen und Anpassungen zu machen. Vier Fünftel der VPP machten Gebrauch von Anweisungen (cues) irgend einer Art, von denen kinesthetische Anweisungen aus Hand und Arm die wesentlichsten waren. Es wurde auch eine Methode der Zeitabmessung (timing) angewendet, mit der die VPP die zur Vollendung der Bewegung von Arm und Hand nötige Zeit abschätzten. Die VPP redeten im Laufe ihrer Strebungen nach Besserung der Leistfrei, und ihr Gespräch (verbalizations) hatte eine direkte Einwirkung auf die Tüchtigkeit der Leistung.

Die Selbstbeobachtungen der VPP und die objektiven Befunde scheinen ganz bestimmt darauf hinzuweisen, dass Verbesserung der Leistung in der Tätigkeit des Linienzeichnens nicht besonders auf die "befriedigenden" (satisfying) und "verdriessenden" (annoying) Einwirkungen der Ausprechung der Wörter "richtig" and "falsch" durch den VLL beruht. Die Verbesserung der Leistung bei dieser Tätigkeit und bei ähnlichen Tätigkeiten wird wesentlich durch die Art und Menge der Auskunft (information), die die VP erhält, bestimmt.

TROWBRIDGE UND CASON

THE RELATION OF THE GALVANIC SKIN REFLEX RECOVERY CURVE TO REACTIVITY, RESISTANCE LEVEL, AND PERSPIRATION*[1]

From the Behavior Research Fund of the Institute for Juvenile Research of Chicago, Illinois

CHESTER W. DARROW

When a succession of stimuli sufficiently strong and varied to prevent adaptation and to give reactions at widely varying resistance levels is given to a subject having moderately high initial resistance,[2] an inverse relationship appears between the resistance level at the time of excitation and the rate of recovery following the respective reactions. It is evident in the case of the subject whose record is shown in Figure 1 that when the resistance is moderately high (40,000 ohms) the recovery of resistance following the peak of reaction is very slight, and that when the resistance has dropped to 30,000 ω the recovery is much more marked, and that when the subject's resistance is only 20,000 ω the recovery rate is quite rapid.

The consistency of this tendency in a group of subjects on whom data are available is shown in the case of 90 university students, mostly freshmen, on whom we have records of physiological changes after a standard series of stimuli, such as sounds (clicks of the apparatus), verbal warning, mild standardized electric shocks, and sounds conditioned to the shocks.[3] As a criterion of the rate of

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[1] Studies from the Behavior Research Fund of the Institute for Juvenile Research, Chicago, Series B, No. 182. We are especially indebted to Miss Lena Heath, assistant in this laboratory, to Miss Reba Grace Gray of the statistical laboratory, to Miss Rosamond Howland and Mr. J. R. Salter of the art department of the Institute for help in the preparation of this material.

[2] Resistance of the palm under a relatively non-polarizable physiological salt, kaolin, zinc sulphate-zinc, electrode 1¼ inch in diameter. The indifferent electrode was of large area (2″ x 3¼″) and was applied to a relatively non-active region, the back of the hand. The resistance of this latter area is generally reduced by pricking with a needle or by the application of iodine.

[3] The conditions of stimulation are described in detail in Darrow and Heath (6). The average number of undisturbed, clearly recorded reactions on which the angle of recovery could be measured was 18.1.

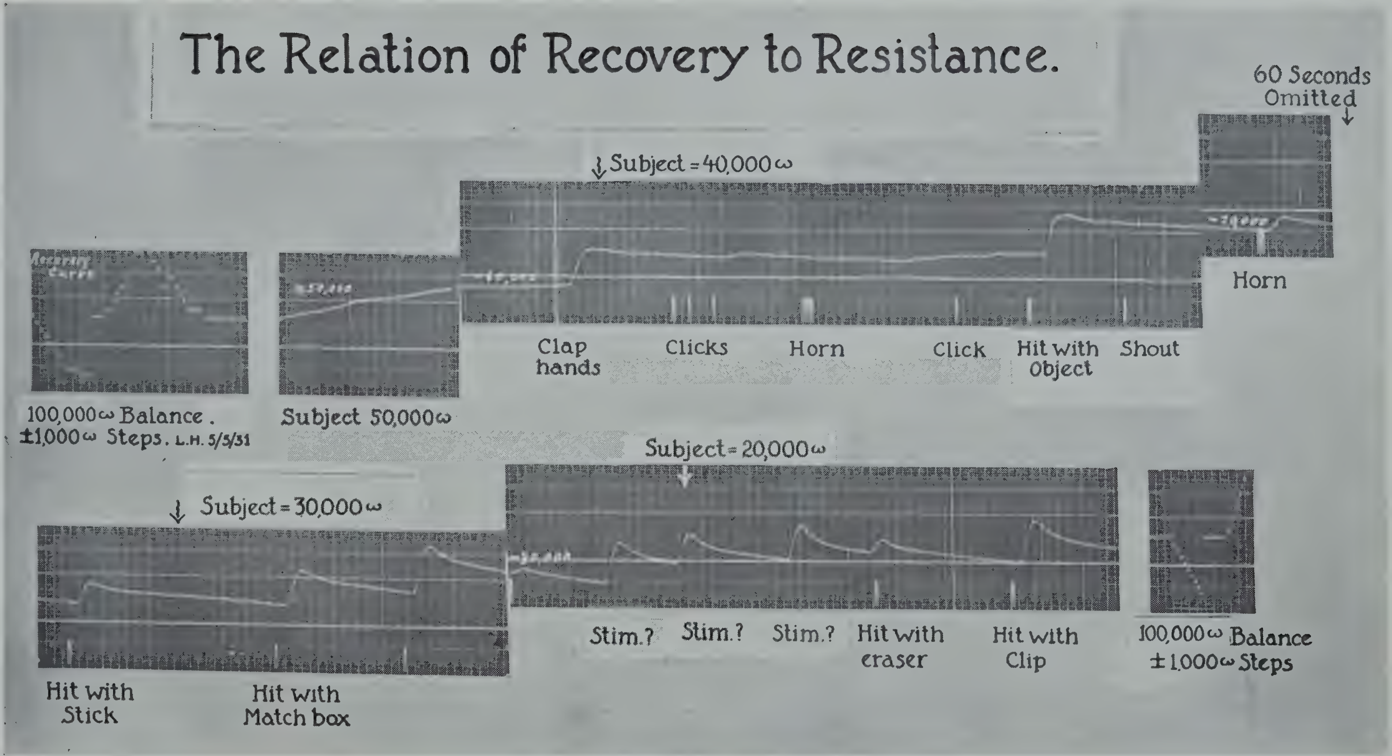

FIGURE 1

Record Showing the Relation of Recovery to Resistance Level

A varied series of stimuli was employed for this illustrative record in order to produce reactions through as wide a range of resistance levels as possible.

The record is obtained with the technic of the Behavior Research Resistance Box. This is a Wheatstone bridge arrangement with resistances respectively of 1,000 ω /10,000 ω and 10,000 ω /100,000 ω balancing the galvanometer. The subject may be introduced as part of the 100,000 ω circuit with a resistance in series with him adjusted to bring the circuit sufficiently near balance for the galvanometer to be within the recording range. Compensation for changes in the subject's resistance tending to carry the galvanometer off the record is made by adjustment of the resistance in series with him. Such compensations (additions of 10,000 ω to the series resistance) have been made at the points where the photographic strip has been cut and displaced to give continuity to the galvanic record. The subject's resistance at any point in the record equals 100.000 ω minus the series resistance plus or minus the deviation from balance indicated by the galvanometer. The scale of standard unit (1,000 ω step) deviations either way from balance is recorded at the beginning and end of the record.

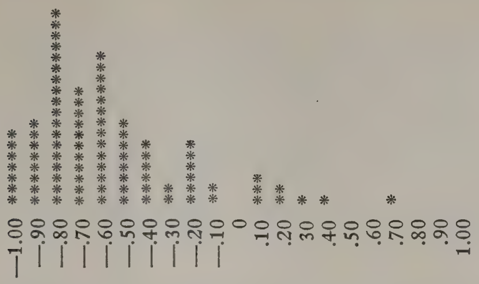

FIGURE 2

Correlations, r, between Angle of Recovery and Resistance Level at the Peak of Reaction, in the Respective Decades between —1.00 and 0, and 0 and 1.00

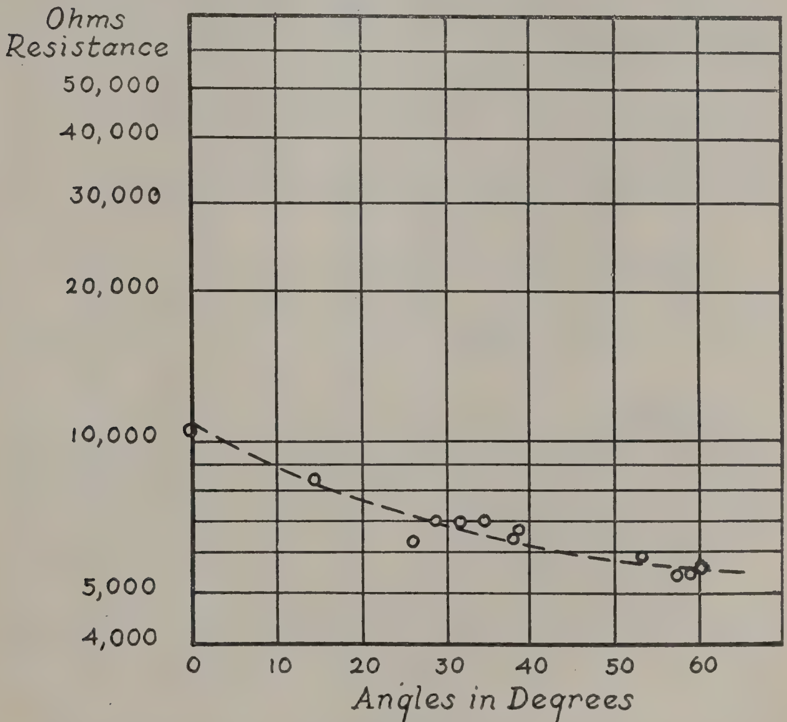

FIGURE 3

The Relation of Angle of Recovery to Resistance Level

Example from a subject (He) selected from the decade in Figure 2 showing the highest correlations ($r=.930\pm.068$).

recovery we have used first the angle of recovery, measured as the angle in degrees between a tangent to the steepest part of the recovery curve, and the base line. We have determined for each subject the correlation coefficients, *r*, between these angles of recovery and the resistances at the peaks of reaction immediately preceding. The distribution of these coefficients in the respective decades between —1.0 and 0, and 0 and +1.0 is shown in Figure 2, where the tendency for the correlations to be significantly negative is readily apparent.

To show the relationship in greater detail we reproduce in Figure 3 one of the graphs illustrative of the result when the angles of recovery are plotted against resistance levels at the peaks of reaction

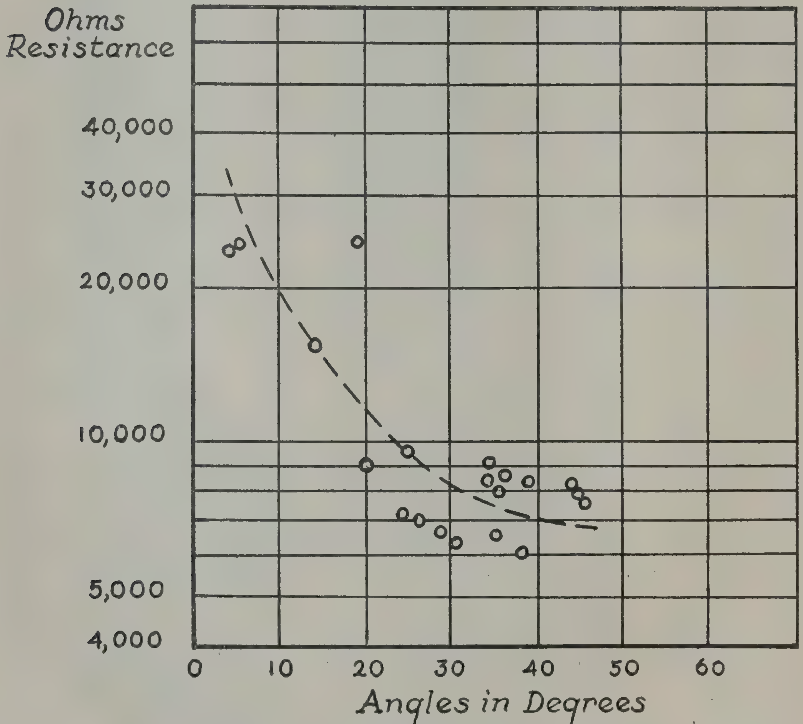

FIGURE 4

THE RELATION OF ANGLE OF RECOVERY TO RESISTANCE LEVEL

Example from a subject (Go) whose correlation coefficient ($r = -.739 \pm .026$) was of modal (most frequent) magnitude.

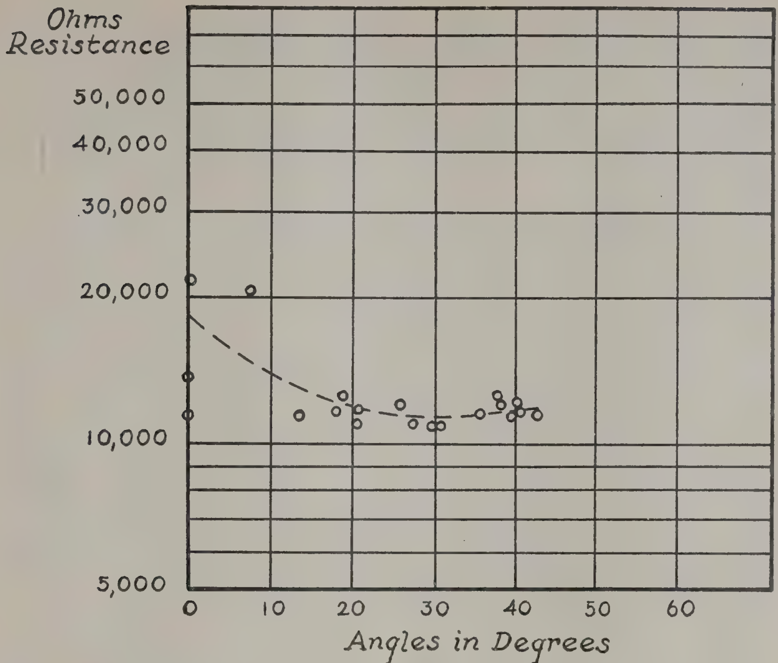

FIGURE 5

THE RELATION OF ANGLE OF RECOVERY TO RESISTANCE LEVEL

Example from a subject (Fe) whose correlation coefficient ($r = -.519 \pm .108$) was approximately of mean magnitude.

for one of the individuals having the high negative correlation coefficients indicated in the left-hand column of Figure 2. A similar graph illustrating the relationship for individuals having correlations between —.70 and —.80 of modal (most frequent) magnitude is given in Figure 4. The data for one of the correlations of approximately mean magnitude are plotted in Figure 5.

The exceptions indicated in Figure 2 in which near zero or positive correlations are obtained are accounted for largely by the fact that these are from individuals having low general resistance level, and by the fact that as resistance approaches its lower limits the absolute magnitude of reaction and recovery necessarily becomes smaller. It is to be expected, therefore, that an individual who apparently approached so near minimum resistance as greatly to reduce reaction might also suffer a reduction in absolute recovery rate and angle of recovery sufficient to counteract the tendency for recovery to vary inversely as resistance level. A method for calculating recovery which partly compensates for this and offers certain other advantages in the comparison of data from different individuals will, therefore, also be used as supplementary to the data on the angle of recovery.

For comparison of different individuals the angle of recovery employed above is an unsatisfactory index due to the fact that it is affected by incidental factors such as the rate of running the record, and the sensitivity of the galvanometer, etc. The change in apparent resistance in absolute units (ohms) is also unsatisfactory for the comparison of the reactions of different individuals owing to the fact (as noted above for the lower limit of resistance change) that as resistance level decreases the magnitude of the electrical response to a given stimulus also decreases. For example, a 10,000 ω change for an individual at the 50,000 ω level may mean no more than a 1,000 ω change for an individual at the 5,000 ω level. The absolute magnitude of the change appears to be significant only in relation to the resistance level at which the change occurs. For this reason the per-cent resistance change has been employed as an index of reactivity by numerous investigators.[4] As an additional index of the rate of recovery we have therefore used the per-cent resistance recovered in three seconds after the peak of reaction:

$$\frac{(\text{Recovery in ohms during three seconds x } 100)}{(\text{Resistance in ohms at peak of reaction})}$$

A three-second interval was selected as long enough to include the steepest portion of the recovery curve for the larger reactions, and small enough not appreciably to exceed the recovery period of the smaller curves.

When per-cent resistance recovered in three seconds is correlated with resistance level at the peak of reaction, higher coefficients are obtained than when the angle of recovery is used. We show the frequency of the correlations within the various decade ranges in Figure 6. These data show a prevailing negative relation between resistance level and recovery rate measured according to this more or less generally accepted method of determining re-

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[4] For example, Brown (1), Lauer (8), Cattell (2), and Slight (9).

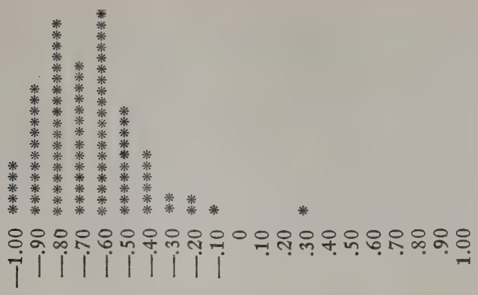

FIGURE 6
Correlations, *r*, between Percentage Recovered in Three Seconds and Resistance at the Peak of Reaction, in the Respective Decades between —1.00 and 0, and 0 and 1.00

activity (per-cent change). For the present purpose, however, this method is open to an objection that the relationship shown might be an artifact, because a negative correlation would be obtained even if absolute resistance change at different resistance levels remained unaltered. This objection is, we believe, justifiable only if the use of per-cent resistance change as an index of galvanic reactivity overcompensates for the fact that the magnitude of resistance change following stimulation decreases as resistance level decreases. Nevertheless, since there is this possible objection, we present the distribution of the correlations obtained according to this method of calculation only as supplementary to the preceding data on the angle of recovery.

The rate of recovery is definitely related not only to resistance level but also to the appearance of microscopically visible perspiration. It has been previously shown (Darrow, 3, 4) that the galvanic reaction in the skin manifests high correlation with electrically recorded moisture in an adjacent area in the center of the electrode. In the present study, by means of micro-moving-pictures of the orifices of the sweat glands and of the simultaneous galvanic deflections it is demonstrated that the galvanic reaction at high resistance levels may occur in the absence of surface visible perspiration, and that the galvanic recovery is a function of the visible sweat.

Light from a *pointolight* lamp is projected past the pointer of a galvanometer and concentrated by a lens in the diaphragm opening of a Zeiss vertical illuminator which projects the illumination and shadow of the galvanometer pointer, reduced in size, upon the microscopic field in the center of an electrode. For convenience in focusing, the electrode and a clamp for the finger are attached to the substage rack and pinion of the microscope. The image of the skin area visible through a hole in the electrode, and the above-mentioned shadow of the galvanometer pointer, are magnified by a short mount objective and projected through a Zeiss *microphote* which splits the light, sending approximately one-tenth to the eye of the operator for observation, and nine-tenths to the photographic film.

Excerpts from a film taken at the rate of eight frames per second are shown in Figures 7 and 8. We show first the record from the finger tip of a subject with high initial resistance, no visible perspiration, and no immediate galvanic recovery after stimulation and reaction. This film is from the same individual whose record is shown in Figure 1. A very low magnification is employed in order that it may be seen that there is no visible secretion in a relatively wide area. The points where globules of moisture later appeared under severe stimulation are indicated by crosses in the second of the enlarged frames. In Figure 7 we show with higher magnification the reaction and recovery in a single sweat gland from the film of a subject at lower resistance with visible perspiration and appreciable recovery.[5] The frames of the 16-mm. film which are numbered have been enlarged for better visibility.

The explanation of the relationship of recovery rate to resistance and surface visible perspiration must await the results of further investigation. It is true that a striking simultaneity of the return movement of the galvanometer pointer and of the disappearance of the sweat globules at the orifices of the sweat glands, shown in the moving-picture films and on direct observation through the microscope,[6] might suggest that there is an active resorption process playing a rôle in resistance recovery. It can be fairly well demonstrated, however, by means of an air-tight chamber, and by observation under oil or glass, that this disappearance of sweat occurs only when there are facilities for evaporation into the air or for spreading into the surrounding film of moisture on the skin.

Another apparent possibility is that recovery is slow or absent in the relatively dry skin because the initial depolarization and drop in resistance initiated in the sweat glands is followed by absorption of excreted moisture into the dry surrounding tissues, increasing their conductivity and thus making the drop in resistance for the entire area relatively permanent. This explanation is not sufficient because it would not apply to the same relationship of resistance to recovery which we have found when liquid electrodes are used and the hand or fingers have for some time been immersed in saline solution.

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[5] The moving-picture film demonstrating this relationship was shown before the American Psychological Association in Toronto (see Darrow, 5).

[6] The secretory and electrical changes here noted are easily observed under low power without the elaborate arrangements here necessary for photographic demonstration. If light from an angle rather than vertical illumination is employed, the dusting of the skin with the powder of a water soluble dye is an aid to visibility.

Without here attempting an explanation, we may suggest that we have here evidence that the resistance changes of the skin conform to principles governing reaction within a fixed range of reactivity varying from a "basal" or resting stage where reaction is followed by little or no recovery, to maximal excitation where immediate recovery approximates the magnitude of the immediately preceding reaction. It may even be that with proper compensation for certain disturbing factors the recovery rate may be used as an index of the degree of reactivity prevailing at a given time. There is also the possibility that this relationship may ultimately afford a means of determining from a few reaction and recovery curves the range of reactivity and the resistances at "basal" and maximal reactivity.

Summary and Conclusions

The above data show:

1. A negative correlation between resistance level and recovery rate for the galvanic skin reflex. This is shown both for the angle of recovery and for the per cent recovered in three seconds.

2. A positive relationship between microscopically visible perspiration and recovery rate.

The possible use of recovery rate as an index of the level of reactivity and for the determination of the "basal" and maximal limits of reactivity in a given skin area is suggested.

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LA RELATION DE LA COURBE DE RÉTABLISSEMENT DU RÉFLEXE GALVANIQUE DE LA PEAU À LA RÉACTIVITÉ, AU NIVEAU DE RÉSISTANCE, ET À LA SUDATION

(Résumé)

Une relation inverse se montre entre le niveau de résistance au temps de l'excitation et la vitesse du rétablissement galvanique qui suit la réaction. Ceci est montré dans un échantillon de la notation photographique et dans les donnés de 90 étudiants universitaires pour lesquels on présente la distribution des coefficients de corrélation entre le niveau de résistance et (1) l'angle du rétablissement et (2) le pourcentage rétabli en trois secondes.

On montre aussi au moyen de cinémicrophotographies des orifices des glandes sudoripares au centre d'une électrode et des déviations de l'ombre d'un appareil enregistreur d'un galvanomètre projeté sur la section examinée que nul rétablissement immédiat n'a lieu après la réaction dans une section qui ne montre pas une sudation visible et possède une grande résistance initiale, et qu'un rétablissement galvanique rapide a lieu dans une section qui montre une grande sudation et une petite résistance électrique. D'ailleurs, une disparition frappante de la sueur visible se montre avec le rétablissement galvanique. On cite de l'évidence pour montrer que la disparition de la sueur n'est pas due à la résorption, mais à l'évaporation et à son expansion dans la couche environnante de moiteur sur la peau.

Le rétablissement galvanique semble se conformer aux principes qui gouvernent la réaction dans la variation entre la réactivité basique et la réactivité maximale. Avec la compensation nécessaire pour certains facteurs contribuants il est possible qu'on puisse s'en servir comme indice du niveau et de la variation de la réactivité.

Darrow

DIE BEZIEHUNG DER ERHOLUNGSKURVE (RECOVERY CURVE) DES GALVANISCHEN HAUTREFLEXES ZUR REAKTIVITÄT, ZUM NIVEAU DES WIDERSTANDES (RESISTANCE LEVEL), UND ZUR AUSDÜNSTUNG

(Referat)

Es zeigt sich eine inverse Beziehung zwischen dem Widerstandsniveau zur Zeit der Reizung und der Schnelligkeit der galvanischen Erholung nach der Reaktion. Diese Beziehung wird erwiesen an einer typischen photographischen Registrierung (sample photographic record) und an Daten, die an 90 Studenten gesammelt wurden. Die Verteilung der aus diesen Daten gewonnen Koeffizienten der Korrelation zwischen dem Widerstandsniveau einerseits und (1) der Spitzheit des Winkels der die Schnelligkeit der Erholung ausdrückt (angle of recovery) und (2) dem Prozentsatz der Erholung (per cent of recovery) innerhalb drei Sekunden anderseits wird wiedergegeben.

Es wird ferner erwiesen, durch Mikrophotographien der Mündungen der Schweisdrüsen in der Mitte einer Elektrode und der Abweichungen des Schattens eines Galvanometerzeigers (galvanometer pointer) der auf die zu beobachtende Fläche projiziert wurde, dass nach der Reaktion in einer Gegend, in der keine sichtbare Ausdünstung stattfindet und die einen starken anfänglichen Wilderstand aufweist, keine sofortige Erholung stattfindet, und das eine rasche galvanische Erholung stattfindet in einer Gegend die starke Ausdünstung und einen schwachen Widerstand der Elektrizität gegenüber aufweist. Ferner wird die galvanische Erholung von einer auffallenden Verschwindung des sichtbaren Schweises begleitet. Es wird Beweis gegeben dafür, dass die Verschwindung des Schweises nicht durch Einsaugung (resorption) sondern durch Verdunstung und durch Ausbreitung in den umgebenden Feuchtigkeitsschleier (film of moisture) verursacht wird.

Die galvanische Erholung scheint mit Grundsätzen übereinzustimmen, die die Reaktion innerhalb des zwischen basaler und maximaler Reaktivität liegenden Feldes beherrschen. Unter passender kompensierender Berücksichtigung gewisser beitragender Einwirkungen kann diese Erholung möglicherweise als Index des Niveaus und der Breite (range) der Reaktivität gebraucht werden.

DARROW

ON THE RATE OF FORMATION OF A CONDITIONED REFLEX*

From the Laboratory of General Physiology of Harvard University

B. F. SKINNER†

I

The establishment of a conditioned reflex is often represented in the following way:

$$\begin{tikzcd}
S \arrow[r] & R \\
S' \arrow[ur, dashed, dash] & R' \arrow[l]
\end{tikzcd}$$

Let $S — R$ and $S' — R'$ be reflexes observable in the organism prior to the process of conditioning, and let there be no observable relation between the stimulus S' and the response R. If the two reflexes are now elicited either simultaneously or nearly so (S' preceding S in the latter case), a correlation between S' and R comes to be observed. A new and "conditioned" reflex, $S' — R$, is established.

A second type of conditioning, in which the developed reflex is originally present and is only strengthened during the process of conditioning, requires another formula:

$$S' — R' \rightarrow S — R$$

Here (to give specific examples) let R be some response leading to the ingestion of food (for instance, a movement of approach to, followed by the seizure, chewing and swallowing of, a piece of food), for which the corresponding S is some part of the several energies emanating from the food. And let R' be some arbitrarily chosen response, the pressing downward of a lever, for example, for which the corresponding S' will be some part of the energies arising from the lever. Both reflexes are observable prior to the conditioning, but $S' — R'$ appears only as a weak "investigatory" reflex, which, if elicited alone, would normally disappear through adaptation. If the response R' is now made to be followed immediately by the stimulus

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S (see the apparatus described below), a change may be observed in the strength of the reflex $S' — R'$, the nature of which is the subject of this paper.

In instances of Type I a new stimulus is substituted in the elicitation of a response, and a new reflex is formed. Two of the original four terms in the paradigm may eventually be disregarded, and one (R') may actually disappear. In Type II, on the contrary, all four terms are retained; the two reflexes are chained together experimentally, and they remain in that relation. No new reflex is established. Types I and II are alike, nevertheless, in requiring as an essential condition for the appearance of the phenomenon the approximately simultaneous elicitation of two reflexes. A further important point of similarity is that the experimental data are in both cases of the same form. What is actually observed in either type is a change in the quantitative relationship of a stimulus and a response—of S' and R in Type I, and of S' and R' in Type II. So far as behavior is concerned, conditioning is definable solely in terms of a change of this sort and of the conditions under which it is observed.

A change in the relationship of a stimulus and a response may be extensive. It may involve the magnitude of the responses correlated with all values of the stimulus, as well as that detailed description of the correlation which is expressed as latency, threshold, after-discharge, and so on (see 2). The state of a reflex with respect to these characteristics may be expressed as its "strength" (2, 3, 4). Experimentally the change can only be sampled. Usually this is done by examining the magnitude of the response correlated with a stimulus having a given constant value (*cf.* Pavlov's method). But this is not always a convenient, and is by no means the only possible, procedure.

The present method has already been used in dealing with a similar change in behavior. When the rate at which a rat eats uniform pieces of a standard food is examined under suitably controlled conditions, it is found to diminish in an orderly manner during an eating period (1, 3). It has been pointed out that a rate of eating is significant only as a measure of certain characteristics of the behavior of the rat, or, more specifically, of the series of reflexes called into play as the rat eats each successive piece of food. It has been shown further that the mode of variation of this rate is independent of the precise nature of the initial reflex in the series, and that the law describing the change in rate applies to such an arbitrary case as that referred to above, where the rat obtains food from a lever-magazine device (4).

Such a change in "hunger," regarded simply as an aspect of the behavior of the rat, is precisely of the sort observed during the establishment of a conditioned reflex. Indeed, so far as the response to the lever is concerned, it is impossible to distinguish between a conditioned non-hungry rat and a hungry unconditioned rat. The response is simply lacking in both cases. Between these two extremes, moreover, it is impossible to tell from a single observation whether a given state of the reflex (an observed strength) is to be attributed to a degree of hunger or to a degree of conditioning. In dealing with a *change* in state, on the other hand, there is little chance for confusion if the conditions of the change are known. "Conditioning," that is to say, and "a change in hunger" differ as processes only with respect to the conditions under which they are observed. The *thing changing* (the observed aspect of behavior) is the same in both.[1] It follows, therefore, that a single method may be employed in measuring the two effects. A single system of coordinates may equally well be used in representing them.

Let a rat with a fairly high degree of hunger be placed in an experimental situation as free as possible from extraneous stimulation and including a suitable arrangement of lever and food-magazine. If the response of the rat to the lever has previously been fully conditioned, it will be evoked immediately, and the elicitation will be repeated at a rate determined only as described elsewhere. This follows by definition of the term "hunger" (3). But if the reflex has not been conditioned, no response will be forthcoming, except as an "investigatory" reflex, when the rate of repetition is negligible for our present purposes. In the latter case let a single response of this investigatory sort be made. Because of the arrangement of the apparatus it will be followed by the delivery of a pellet of food (which emits the already conditioned stimuli for a series of reflexes leading to the ingestion of the pellet), and some degree of conditioning of the response to the lever will take place. If the conditioning is *complete* upon this one occasion, the elicitation will subsequently be repeated at a rate determined only as above, barring the operation of some other variable of which the rate is a function. If, on the other

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[1] The direction of the change is not a significant differentia, since both processes are reversible.

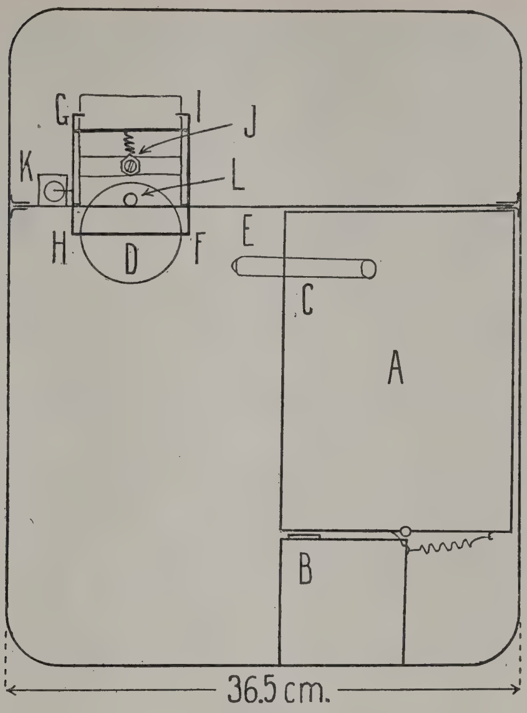

FIGURE 1

Diagram of the Experimental Box and the Apparatus Contained Therein
For description see text.

hand, the conditioning is only partially complete, a lower rate will be observed (resembling, for example, the rate for a fully conditioned response at a lower level of hunger). This is the essential feature of the present method. The rate of elicitation is taken as a measure of the strength developed in the reflex in the course of the conditioning.[2]

II

A suitable food-tray, magazine, and lever have been described elsewhere (4). The present apparatus differs in several respects. The conditioning takes place in a small experimental box, 45 cm. x

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[2] For the interpretation of a rate of eating in terms of strength see (3) and (4).

36.5 cm. x 23 cm., carefully soundproofed and well ventilated. In Figure 1 the contained apparatus is shown schematically as if projected upon the floor of the box. The rat is carried to the experiment in a small cage, which fits into place at *A*. The rat may be released from the cage, after the box has been closed, with a magnetic latch (*B*). An inverted metal cone (*D*) serves as food-tray. Its rim is 4.5 cm. from the floor. Directly above the cone is a horizontal section of heavy wire (*HF*), 8 cm. in length, forming part of a lever, the fulcrum of which is at *G* and *I*. The lever is suspended by the adjustable spring (*J*) and is at rest when the section (*HF*) is 9.5 cm. from the floor. The horizontal section of the lever will move downward approximately 1.5 cm., and at half this distance a circuit is completed at a mercury cup (*K*). A weight of 10 grams will force the lever into the required position. When a contact is so made, a magazine (described in 4) releases a pellet of food into the tray through a tube (*L*). (The food is a commercial dog-biscuit, ground to a fine powder and made into uniform pellets with a pill-machine, as described in 4.) A tube of water is shown at *C*. The tray, the horizontal section of the lever, and the tube of water are accessible to the rat; the rest of the apparatus is closed off by the wall (*E*). Contacts made by the rat are recorded as described elsewhere (4). The accompanying figures are reproduced directly from kymograph records. Each elevation of the writing point indicates one contact.

Four essentially identical sets of this apparatus and four male rats, 110 days old on the day of the experiment, were used. Two of the rats (KI1 and KI2) were members of the 69th generation of an inbred strain. The others (P1 and P2) were members of the 18th generation of another inbred strain. The animals were adapted to the apparatus during a preliminary period of more than two weeks, during which they were placed in the boxes at the same time daily for a period of two hours and permitted to eat their daily ration of food (dog-biscuit of the kind used in making the pellets). For four other daily (but shorter) periods they received pellets of food from the magazines, which were operated by the experimenter from an adjoining room. One hundred forty pellets were received in this way altogether. At the end of this preliminary training the animals had been thoroughly conditioned to respond to the sound of the magazine (a clear click) by approaching the food-tray and

TABLE 1

INTERVALS ELAPSING BETWEEN THE FIRST TEN RESPONSES TO THE LEVER

Interval	P1	P2	KI1	KI2
Release — 1	1 hr. 24′ 10″	4′ 40″	5′ 00″	6′ 20″
1 — 2	1′ 27″	25″	56′ 27″	25″
2 — 3	44″	29″	47′ 33″	15″
3 — 4	12″	26″	24′ 53″	18″
4 — 5	22″	22″	43″	17″
5 — 6	18″	16″	21″	10″
6 — 7	19″	33″	28″	30″
7 — 8	20″	21″	10″	15″
8 — 9	18″	22″	10″	17″
9—10	18″	18″	15″	17″

seizing the pellet of food. (The sound of the magazine thus becomes the *S* of the second formula for conditioning.)

Through this preliminary period the lever had been detached from the supporting spring and rested at its lowest point. On the following day it was suspended in its proper position, so that for the first time it could be pressed downward and would then deliver a pellet of food from the magazine. The animals were placed in the boxes at the usual hour and released. The four records are reproduced in Figure 2.[3] They are supplemented by Table 1, which gives the intervals elapsing between the first ten responses in each record, measured independently at the time of the experiment.

III

Each curve in Figure 2 is resolvable into two parts: an initial positive acceleration of short duration, followed by a more extensive negative acceleration. The latter reflects a change in hunger and may be ignored here. A significant difference in slope between the curves for the two strains is typical for the normal eating curve and may also be disregarded.

The curves for KI2 and P2 begin at very nearly a maximal slope. A slight initial acceleration is nevertheless observable, with an inflection point lying at about the twelfth response in each case. A similar acceleration is evident in the record for P1, but the initial rate (determined by the first two responses) is relatively low. The acceleration is much greater, and the inflection point falls at only the

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[3] A fault in the apparatus prevented the release of P2. Its record was obtained on the following day.

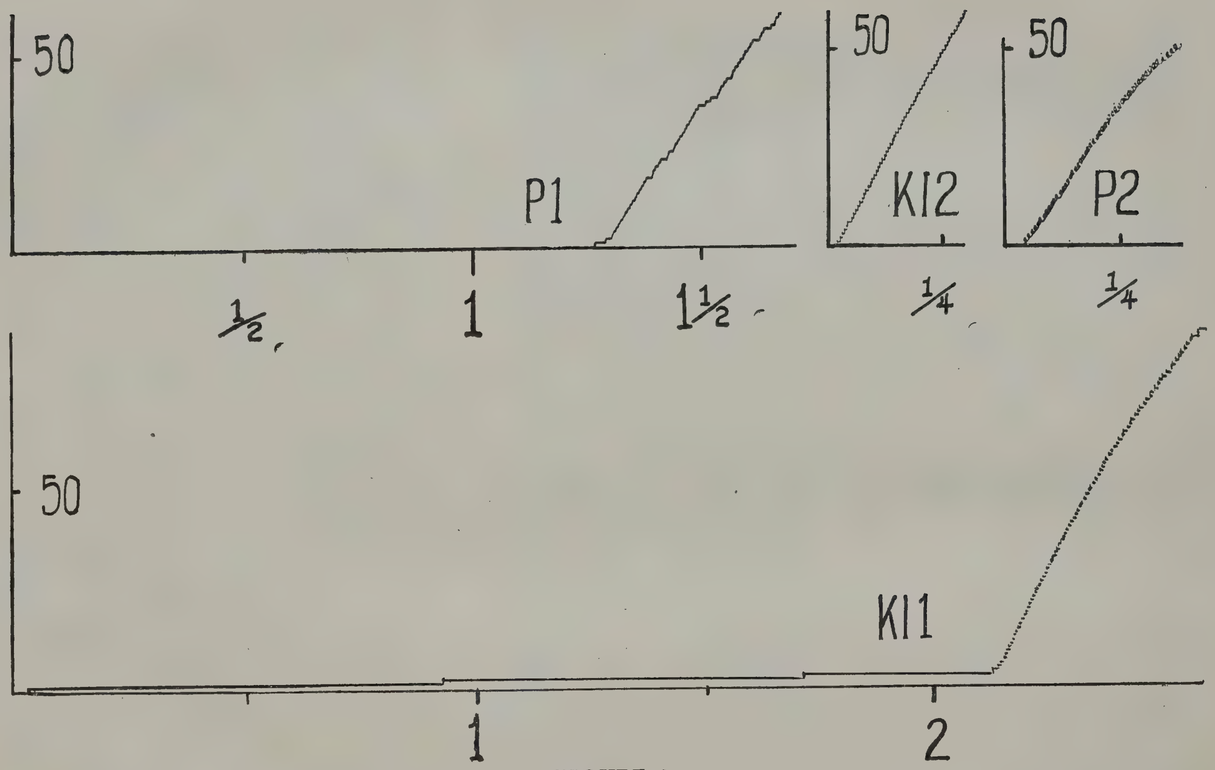

FIGURE 2
Abscissas: time in hours. Ordinates: number of responses to the lever.
For discussion see text.

TABLE 2
MAXIMAL RATES EXPRESSED IN RESPONSES PER MINUTE

	P1	P2	KI1	KI2
Dec. 10	*3.2*	—	*3.8*	*3.7*
Dec. 11	4.1	*3.0*	5.0	4.3
Dec. 12	4.5	4.8	5.7	4.9
Dec. 13	4.8	5.0	6.1	5.5

third or fourth response. In the record for KI1 the first three responses are apparently ineffective, although they may be regarded as establishing a very low initial rate. Beginning with the fourth response the curve closely resembles that for P1.

The maximal rates exhibited in the four records are given as the first entries in Table 2, where they are compared with the maximal (and initial) rates attained on subsequent days. A progressive daily increase in the maxima may be observed. This is possibly to be regarded as an increase in hunger, for which the feeding conditions of the experiment were favorable, but another interpretation is noted below. The comparatively low maxima on the day of the conditioning probably cannot be accounted for in this way, and it seems necessary to conclude that the maximal rate expected of a fully conditioned rat was not observed in any of the four cases.

There is nothing gained in setting an arbitrary point at which conditioning shall be regarded as complete, for we are not interested in a qualitative statement of that sort. It is possible, moreover, that complete conditioning may only be approached as a limit. Nevertheless it is evident from the records for P2 and KI2 that *the greater part of the change with which we are concerned takes place practically instantaneously upon the first occurrence of the* $S' - R' \rightarrow S - R$ sequence. Any observed subsequent increase in rate is virtually negligible compared with the acceleration occurring at the first response.

So far as our observations are concerned, we can mean by "complete conditioning" only that no further appreciable increase in the strength of the reflex can be observed correlated specifically with the occurrence of the $S' - R' \rightarrow S - R$ sequence. This does not mean that further changes in strength may not appear, provided other variables are present. We have thus far assumed that the rates recorded in the present case were determined *only* by a degree of hunger and a degree of conditioning. If we examine the experiment closely, however, we shall discover other factors that must have affected the rate. It can be shown, in fact, that a depressed initial rate and a subsequent positive acceleration are to be expected, even though conditioning is, in effect, complete at the first response.

In the normal eating behavior of the rat the administration of any stimulus to which adaptation has not taken place will normally depress the rate of eating. An example of this inhibitory effect has been noted in (3). In the present case special care was taken to eliminate incidental stimulation. The animals were in the dark in soundproofed boxes. Adaptation to tactual and olfactory stimulation and to whatever auditory stimulation was generated by the rat itself must have been very nearly complete during the preliminary period. Nevertheless, certain "new" stimuli were present when the lever was pressed for the first time—tactual stimuli arising from the movement of the lever and auditory stimuli from the contact of the lever at its end positions. It is also doubtful whether the flash from the mercury contact was adequately screened. These are, it is true, relatively weak stimuli. Their effect upon a normal eating curve would not be very great. But the effect is in the right direction to account (if only in part) for the present records. Such an inhibitory action will account also for the progressive daily increase in maximal rate, if some "loss of adaptation" during the twenty-two or more hours between successive experiments is assumed. Such a loss may easily be demonstrated.

There is a more important reason for expecting a submaximal rate at the beginning of these records. It has been noted elsewhere (4) that to speak of *a* response to a lever is a rather severe though necessary simplification. We are actually concerned with an indefinite number of responses, corresponding to the number of possible ways in which the stimulating energies may affect the organism. "We are justified in speaking of *one* initial reflex only because the members of this indefinite group possess very much in common." We have seen why this diversity does not affect an observed rate of eating *after* conditioning has occurred. During the process, however, it must be taken into account.

We are concerned with all those responses of the rat that produce the required movement of the lever. Let us suppose that the stimuli corresponding to these responses are composed of a number of elements $A, B, C, \ldots . N$ (the actual number being very great). The rat cannot be acted upon at a given moment by more than a part

—let us say four—of these elements. And let us suppose that upon the first occurrence of the $S' — R' \rightarrow S — R$ sequence the S' is actually composed of elements *A, C, D, J.* These, and no others, will share in the conditioning. If, after the pellet is eaten, the rat again falls under the action of *A, C, D, J,* the response will be immediately repeated (assuming the conditioning to have been complete and disregarding the effect of incidental stimulation). But we cannot completely control the stimulus in such a situation, and it is more probable that some other group, say *B, D, E, G,* will be met with. Part of this group (the element *D*) has now been conditioned. After some delay but aided by the element *D,* the second response is evoked, and the elements *B, E,* and *G* become conditioned also. Such a system has, roughly speaking, the following properties: (1) The complete set of elements will eventually become conditioned stimuli. We may then deal with the behavior of the rat as if there were *one* response to the lever. (2) As this state is approached, any selected group will tend to contain more of the conditioned elements. Consequently the rate of elicitation will show a positive acceleration. (3) Although any single interval between two responses will be determined by factors not under control, any deviation from the expected rate will compensate for itself. If, for example, the second group possesses most of its elements in common with the first, the response will be elicited without much delay, but the conditioning of the remaining members of the whole set will not be advanced very far. On the other hand, if the second group is composed mainly of unconditioned elements, the response will be retarded, but the conditioning of the set will be greatly furthered.

In the present case most of the possible responses that will move the lever in the required way possess a great deal in common. When a single group of elements has been conditioned, almost any other group will also have been conditioned to a great extent. Consequently, we expect only a slight depression in rate, requiring only a slight subsequent acceleration—which, as a matter of fact, we observe. There will be certain extreme cases, however, and these may conveniently be appealed to to account for the first three responses of rat KI1.

IV

To what extent the effect of inhibitory stimuli and the diversity of the responses to the lever account for the characteristics of the present records can be determined only by further experimentation. It seems improbable that they should not have played some appreciable part. And whatever effect they may have had, it need not have been very great to force the conclusion that for a given stimulus the process of conditioning was essentially complete with a single response.

If this be true, it raises an interesting problem of method. We can measure the strength of a reflex only by eliciting it. In the case of conditioning we are therefore limited to two measurements—one taken before, and one at the time of or after, the event. It is therefore impossible to determine the *course* of the process. To put this in another way, we can represent the process of conditioning by plotting the strength of the reflex, not against time, but against the number of elicitations (since we can make determinations only at these points). If the entire process takes place at one elicitation, the graph will be discontinuous. The present experimental situation, however, was especially designed to favor rapid conditioning. It should not be difficult, with some suitable change in conditions, to retard the process. In that case the rate observed in the present case will give a satisfactory base line upon which to note the effect of such a manipulation of the independent variables governing the phenomenon.

SUMMARY

In the establishment of a conditioned reflex the observed datum is a change in the quantitative relationship of a stimulus and a response. Prior to the process of conditioning, the stimulus and the response may have been totally unrelated (Type I) or in some way already related (Type II). In either type the process may be dealt with as a change in reflex strength.

A method for investigating such a change is described, and data for four rats are presented. The process of conditioning may be essentially complete at one elicitation. A maximal strength is not observed immediately following the event, but the observed strength is accounted for on the following grounds. Certain "new" stimuli were present upon the first elicitation that would normally have depressed the strength in the observed manner. Secondly, it is a simplification to speak of a single response. Actually a group of many very similar responses is being dealt with. When this diversity is considered, the submaximal strength here observed can reasonably be accounted for.

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SUR LA VITESSE DE LA FORMATION D'UN REFLEXE CONDITIONNEL

(Résumé)

Dans l'établissement d'un réflexe conditionnel la donné observée est un changement dans la relation quantitative d'un stimulus et d'une réponse. Avant le processus du conditionnement le stimulus et la réponse ont pu être totalement sans relation (Type I) ou déjà en quelque relation (Type II). Dans les deux types on peut étudier le processus comme un changement de la force du réflexe.

On décrit une méthode pour l'investigation d'un changement du Type II. La vitesse à laquelle on fait sortir la réponse sous une stimulation continue est prise comme mesure de la force développée au cours du conditionnement. On présente des données pour quatre rats. Le processus du conditionnement peut être essentiellement complet à la première réponse. On n'observe pas une force maximale directement après l'évènement, mais on explique la force sous-maximale observée par les raisons suivantes. Certains stimuli "nouveaux" ont été présents à la première réponse lesquels auraient déprimé la force normalement de la manière observée. Deuxièmement, c'est une simplification de parler d'une seule réponse. Vraiment on étudie un groupe de plusieurs réponses très semblables. Quand on considère cette diversité, on peut expliquer très raisonnablement la force sous-maximale ici observée.

SKINNER

ÜBER DIE SCHNELLIGKEIT DER BILDUNG EINES BEDINGTEN REFLEXES

(Referat)

Das bei der Gründung (establishment) eines bedingen Reflexes beobachtete Datum besteht aus einer Änderung in der quantitativen Beziehung zwischen einem Reiz und einer Reaktion. Vor dem Vorgang der Bedingung (conditioning) können Reiz und Reaktion vollkommen ohne Beziehung gewesen sein (Typus I) oder schon auf eine oder die andere Weise in Beziehung gestanden haben (Typus II). Bei jedem der beiden Typen kann der Vorgang als Änderung in der Stärke des Reflexes behandelt werden.

Es wird eine Methode zur Üntersuchung einer Änderung des Typus II beschrieben. Die Schnelligkeit, mit der die Reaktion bei ununterbrochener Reizung hervorgerufen wird, wird als Massstab der im Lauf der Bedingung entwickelten Stärke angenommen. Es werden an vier Ratten gesammelte Daten vorgelegt. De Vorgang der Bedingung kann im Wesentlichen nach einer einzigen Hervorrufung vollmässig entwickelt sein. Eine maximale Stärke wird nicht sofort nach dem Vorgang beobachtet; die aber beobachtete unter-maximale Stärke lässt sich aus folgenden Gründen erklären. Es bestanden zur Zeit der ersten Hervorrufung gewisse "neue" Reize, die normalerweise die Stärke auf die beobachtete Weise gehemmt hätten. Zweitens, vereinfacht man zu sehr, wenn mann von einer einzelnen Reaktion spricht. In Wirklichkeit hat man es mit einer Gruppe bestehend aus vielen sehr ähnlichen Reaktionen zu tun. Berücksichtigt man diese Mannigfaltigkeit, so lässt sich die hier beobachtete unter-maximale Stärke verstanding erklären.

Skinner

COMPARATIVE SUGGESTIBILITY IN THE TRANCE AND WAKING STATES—A FURTHER STUDY*[1]

From the Psychological Laboratory of the University of Arizona

J. E. Caster and C. S. Baker, Jr.

Hull and Huse (2) offer quantitative evidence based on results secured with the postural sway test that the hypnotic trance is a condition of heightened suggestibility as compared with the normal waking life. They have also shown that when a second suggestion is begun two minutes following the termination of the reaction to a previous suggestion, a 25-per-cent reduction appears in the time required to evoke a reaction. This was found characteristic of consecutive reactions in both the trance and normal states. Williams (3), using the same apparatus and essentially the same technique, secured similar results. It has been suggested that a general principle may be operative in these experiments, viz., that response to one direct suggestion increases the susceptibility to further direct suggestion, regardless of the nature of the acts suggested. Thus the procedure of evoking closure and ultimate catalepsy of the eyelids would be responsible, at least in part, for the state of heightened suggestibility which is so characteristic of the hypnotic trance. In a similar manner, positive response to any other direct suggestion, such as to postural movement, ought, where hypnosis is induced soon after, to facilitate the closure of the eyes and the onset of the trance state in general. Unfortunately, in neither of the above studies was a record made of the time required to evoke permanent lid closure in the successive trance periods, so that evidence concerning this important possibility was not secured. Since such data may be of considerable significance in determining the nature of hypnosis and suggestibility, the present experiment was designed to secure such

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[1] The investigation here reported was facilitated by a portion of a grant-in-aid originally assigned by the Social Science Research Council to Professor C. L. Hull. In addition, the authors are indebted to Dr. Hull for suggesting the problem and for the design of the novel portion of the apparatus employed in the experiment.

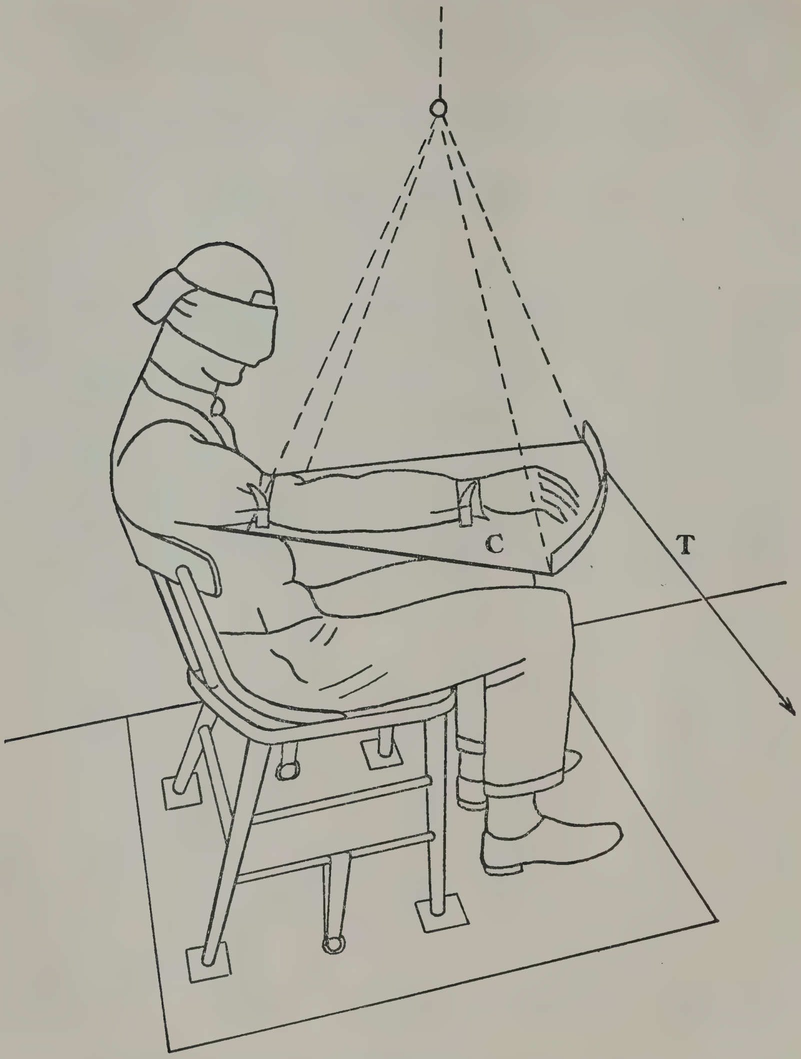

FIGURE 1

THE EXPERIMENTAL SET-UP

Showing the appearance of the arm-cradle (*C*) with the subject ready to respond to suggestion. *T* indicates the thread which communicates with the recording apparatus.

records along with further evidence concerning the fundamental question of the relative suggestibility in the trance and waking states.

Apparatus and Procedure

In place of the postural sway test employed by Hull and Huse, by Williams, and others, there was substituted the horizontal sway of the arm which was supported in a special cradle devised by Hull. This consists of a light triangular wooden platform suspended by a light chain which passes through a smooth-running pulley secured to the ceiling. The general nature of the arm support and the mode of its use are shown in Figure 1. The height of the platform was nicely adjustable to the height of each subject's arm by means of a hook at the end of the portion of the chain descending from the pulley which could be inserted into any desired link of the ascending portion. The upright curved sheet-metal plate at the outer extremity of the horizontal triangular portion of the platform was fashioned to conform to the arc of a circle of 30-inch radius which had its center at the shoulder joint. To a small brass eyelet secured to the left (front) side of this plate was attached a thread which passed backward to the recording apparatus about 4 feet from the subject. As the platform or cradle swung to the left, the thread came into progressive contact with the outer convex surface of this plate and thereby avoided certain distortions of the resulting record which would have taken place had the thread been attached to the end of a simple revolving lever. The apparatus also avoids the difficulty of different arm lengths among the subjects by making the radius of movement equal for all.

The recording apparatus has been fully described by Hull (1). The record is traced by the stylus of this apparatus on the kymograph drum in the form of a vertically oscillating line reduced to one-third the actual distance moved by the sheet-metal plate. The appearance of these tracings, while less complicated by tremors, is substantially the same as those secured in other experiments involving the use of this instrument for recording postural movements.

The kymograph and a small tripod support to which was clamped a Jacquet signal chronometer and a magnetic signal marker rested on a small low table. The former traced a time line in seconds. A second, but larger, tripod support held the Hull recorder just mentioned. The three styluses were kept in vertical alignment at all times.

In order the more rigidly to control experimental conditions, both of the authors took part in every experimental determination. The recording thread passed directly before the eyes of the second experimenter on its way from its point of attachment on the sheet-metal arc of the cradle to the aluminum pulley of the recording instrument. Two brass arrows, 8 inches apart, were secured to the large tripod support, and in line with this thread. Previous to the beginning of suggestions the subject's arm in the supporting cradle was moved to the latter's right until it assumed a position roughly 45 degrees from his straight front. A small mark, placed on the thread for the purpose, was then directly in front of one arrow. The subject's response to suggestions of arm movement swung the cradle to the left, thus pulling forward the thread attached to it. Suggestions were continued until the mark on the thread reached a point opposite the second arrow. If the subject failed to move the arm the required distance within the time limit of 3 minutes the suggestions were terminated. Nearly all of the subjects moved the arm the full 8 inches within that time. Special treatment of the data in those cases which failed to move the arm the required distance will be described later. A key controlling the electromagnetic marker was attached to the wall at the right of the second experimenter, who gave the suggestions. The key was closed at the beginning of the suggestions and at their termination. The mark on the smoked paper record made by the former was the point of reference for most of the time measurements to be discussed later.

The greatest care was exercised to keep the words, tempo, and emphasis of the suggestions constant at all times. The same experimenter always gave the suggestions to a given subject throughout the latter's connection with the investigation. To avoid the possibility of a constant error entering the experiment through an unconscious influence on the experimenter's voice from watching the progress of the subject's reactions, the one who gave the suggestions stood, while doing this, in such a position that he could see neither the subject nor the recording apparatus. His cues for beginning and terminating the suggestions were arm signals made by the second experimenter, who observed both the subject and, particularly, the progress of the marked portion of the thread over the 8-inch distance between the two arrows.

The experimental room was located in the basement of a building which is unusually quiet and free from disturbance in the evening.

Since true practice effects, if any, could be shown satisfactorily only under conditions of equal spacing of the experimental periods, each subject reported at the laboratory at exactly the same hour on four consecutive evenings. All of the experimental work was performed during March, 1932.

Ten subjects were used, of whom five were male and five female. They were all students in the University of Arizona, and ranged from freshman to graduate in rank. The youngest was 18 years old, and the eldest 22, the average being 20.1. None of the subjects received compensation for his services. The subjects underwent several training periods during the two weeks previous to the experiment. Each was trained to go into the trance by the experimenter who later gave the suggestions in the experiment proper.[2] All evinced complete amnesia for events occurring during the trance, and all remained in total ignorance of the purpose of the investigation.

The ten subjects were divided arbitrarily into groups of five each. The arrangement of the program of administering suggestions followed exactly that employed by Hull and Huse. In the present experiment, an A day followed the sequence: *trance, trance, waking, waking;* while the B day followed the sequence; *waking, waking, trance, trance.*

On arrival at the laboratory the subject was requested to rest a few moments and was then conducted to the experimental room where he was seated in a straight-backed chair secured by straps to eyebolts in the floor (Figure 1). The detailed procedure on an A day was as follows: The subject was seated in the chair in a relaxed position and immediately put into a trance by the following verbal suggestions:

> "You're perfectly relaxed; you're perfectly relaxed. There is no tension; every muscle in your body is relaxed. You're perfectly relaxed now. You're going to sleep and your eyes are closing now. (Your eyes are closing. You can hardly keep your eyes open now. Your eyes are heavy; your eyes are closing, closing. Tight closed now. You're sound asleep, perfectly relaxed, sound asleep. You're in a deep, relaxing

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[2] During the training period all of the subjects were put into the trance state by the method of optical fixation, during the experiment proper verbal suggestions only were given.

> sleep, a deep, sound, peaceful sleep. Perfectly relaxed, sound asleep, perfectly relaxed; deep, sound sleep.)"[3]

A split-second stop-watch was used in taking the time required to secure permanent closure of the eyes. Those familiar with trance induction know that the eyes may flicker and close several times before final catalepsy of the lids occurs. With the stop-watch employed, a small button on the side of the watch can be pressed each time the lids close, which stops the upper of the two hands while the lower hand goes on without interruption. In case the lids open again, the button is merely pressed a second time and the upper hand leaps to whatever position the lower happens to be at the time, and they move forward together until the lids close again, when the button is pressed a second time, and so on. The reading of the upper hand at the point where it was stopped for the closure that finally proved permanent is recorded. In this way may be secured an accurate timing of this somewhat elusive reaction.

As soon as the subject was in the trance the blindfold was adjusted and the arm was secured to the cradle by straps. The following suggestions began immediately:

> "Your right arm is moving to the left. Your arm is moving to the left. It's moving now, faster, faster. There it goes now; it seems to move all by itself. (Your arm is moving to the left, it's moving, it's moving to the left. It's moving to the left faster, faster. It's moving, it's moving.)"

When the arm had moved the required distance, or at the expiration of 3 minutes, it was swung back to the starting position and the following suggestions were given:

> "Perfectly relaxed, perfectly relaxed. Your arm is going to remain there for a moment now. Your arm is going to stay in this position for a moment now."

Exactly 2 minutes after the completion of the first trance suggestion the same suggestions were repeated. After the completion of the response to this second suggestion, the arm was returned to its starting position, removed from the cradle, and the subject was relieved of the blindfold. He was instructed to pass to the adjoining room and move about or read for 15 minutes. Near the end of that time he returned to the experimental room and was again seated in the chair. In the 16th minute after the instant of waking him from the trance the experimenter engaged him in casual conversation for a period of exactly the same length as was required to put him into the trance. The blindfold was then adjusted as before, and the arm strapped once more in the cradle. Then followed immediately exactly the same suggestions for arm movement given above for the trance state. Completion of response to the first suggestion was followed by an interval of exactly 2 minutes as before. After completion of the response to the second suggestion the blindfold was removed and the experimenter engaged the subject in casual conversation for a period equal to that required to waken him from the trance. He was then excused from the laboratory.

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[3] If the subject was not already in the trance at this point, the portion of the suggestions enclosed in parenthesis was repeated.

On the B days the sequence of suggestions was reversed.

RESULTS

Complete data secured from direct measurements of the records of the ten subjects will be found in Table 1. From these data it is possible to determine the following results:

1. The time required to secure permanent closure of the lids when inducing the trance, or *lid-closure time.*
2. The time which elapses between the beginning of suggestions for arm movement and the beginning of arm movement, or *latent time.*
3. The effect of the trance upon the suggestibility of the subject as measured by the duration of suggestion required to move the arm apparatus eight inches, or the *comparative suggestibility.*
4. The effect of previous stimulation in a given state upon subsequent stimulation in the same state when all other conditions are kept constant and only a short interval exists between, or the *cumulative effect.*
5. The effect of stimulation in one state upon similar responses in a second state subsequent to the first, or the *perseverative effect.*
6. The possible effect of training on both the four consecutive experimental days and the four experimental periods of one day, or the *practice effect.*

On the first and second experimental days subject F.R. failed to move the arm a measurable distance in the normal state. Subject G.C. failed to move the arm on six of the eight tests in the normal state. In these cases it was necessary to interpolate the response time, as indicated in Table 1. Also, during several of the trance

TABLE 1

This shows for every subject in detail the suggestion time and the latent period for every trance and waking period, and the lid-closure time for every trance induction. All scores are in seconds. *L.T.* = latent time, i.e., that elapsing before any movement takes place; *S.T.* = suggestion time, i.e., that required to move 8 inches.

Group	Subject	Date	Type of day	Waking reactions Arm trial I L.T.	Waking reactions Arm trial I S.T.	Waking reactions Arm trial II L.T.	Waking reactions Arm trial II S.T.	Trance reactions Arm trial I L.T.	Trance reactions Arm trial I S.T.	Trance reactions Arm trial II L.T.	Trance reactions Arm trial II S.T.	Lid-closure time
I	F.R.	3-15	A	63	100*	30	100*	7	13	6	9	6.0
		3-16	B	51	100*	5	100*	6	14	5	9	1.4
		3-17	B	7	67	6	32	4	10	3	10	1.8
		3-18	A	1	28	4	35	4	7	3	5	0.0
	A.S.	3-15	A	0	148	0	13	26	73	0	16	37.2
		3-16	B	0	142	0	31	7	36	0	13	24.0
		3-17	B	1	30	0	66	0	15	4	14	20.6
		3-18	A	0	64	8	47	4	17	4	15	15.0
	C.L.	3-15	A	2	10	1	9	4	14	1	8	16.0
		3-16	B	5	12	6	11	0	15	5	21	17.0
		3-17	B	1	10	0	10	0	19	5	19	14.6
		3-18	A	0	12	3	16	3	12	0	16	16.4
	A.F.	3-15	A	13	27	0	18	1	32	1	22	38.6
		3-16	B	11	19	12	17	8	14	3	12	30.0
		3-17	B	1	22	3	22	3	6	7	10	32.0
		3-18	A	5	15	5	11	4	9	5	6	24.0
	R.J.	3-15	A	1	51	1	43	5	38	1	57	17.0
		3-16	B	0	36	1	31	5	50	0	62	14.0
		3-17	B	6	34	3	36	9	42	0	42	17.4
		3-18	A	4	48	3	49	12	45	11	48	15.6

TABLE 1 (*continued*)

				Waking reactions				Trance reactions				
			Type of	Arm trial I		Arm trial II		Arm trial I		Arm trial II		Lid-closure
Group	Subject	Date	day	L.T.	S.T.	L.T.	S.T.	L.T.	S.T.	L.T.	S.T.	time
II	V.V.	3-20	B	4	17	4	32	10	14	6	11	30.0
		3-21	A	4	7	5	8	5	15	5	13	22.0
		3-22	A	4	7	4	9	5	10	5	10	26.0
		3-23	B	3	6	3	6	7	7	5	7	19.0
	R.L.	3-20	B	15	82	7	41	8	28	4	20	31.0
		3-21	A	23	73	22	61	14	50	20	42	29.0
		3-22	A	20	42	7	32	15	45	12	34	25.0
		3-23	B	16	32	5	20	6	18	5	16	17.0
	V.H.	3-20	B	8	180	23	174	2	34	4	28	47.4
		3-21	A	16	154	7	101	9	63	3	49	69.0
		3-22	A	5	100	5	87	9	61	5	46	45.0
		3-23	B	7	138	0	109	4	63	0	73	48.0
	M.M.	3-20	B	14	73	5	60	11	24	6	17	33.0
		3-21	A	12	26	2	19	5	15	5	14	26.0
		3-22	A	5	13	1	8	9	45	2	6	23.0
		3-23	B	3	11	2	8	6	19	3	16	23.0
	G.C.	3-20	B	14	26	41	100*	10	30	50	88	241.0
		3-21	A	41	100*	54	100*	15	42	12	58	22.0
		3-22	A	128	100*	18	149	14	18	31	47	0.2
		3-23	B	130	100*	27	100*	24	27	44	156	19.0
Sums				644	2262	333	1921	300	1119	291	1165	
Means				16.10	56.55	8.33	48.03	7.50	27.98	7.28	29.13	

*Values interpolated.

TABLE 2

SUMMARY OF THE LID-CLOSURE TIMES TO DETERMINE WHETHER HYPNOSIS IS INDUCED ANY MORE READILY WHEN FOLLOWING TWO RESPONSES TO SUGGESTION IN THE NON-TRANCE STATE THAN WHEN NOT SO PRECEDED. SCORES ARE IN SECONDS

Subject	Sum of two lid-closure times when trance is not preceded by waking suggestions	Sum of two lid-closure times when trance is preceded by waking suggestions	Reduction in lid-closure time when trance is preceded by waking suggestions
F.R.	6.0	3.2	+ 2.8
A.S.	52.2	44.6	+ 7.6
C.L.	32.4	31.6	+ .8
A.F.	62.6	62.0	+ .6
R.J.	32.6	31.4	+ 1.2
V.V.	48.0	49.0	— 1.0
R.L.	54.0	48.0	+ 6.0
V.H.	114.0	95.4	+ 18.6
M.M.	49.0	56.0	— 7.0
G.C.	22.2	260.0	—237.8

tests the subjects just mentioned, together with subject A.S., failed to move the arm the required 8 inches. In order to make their records comparable, a shorter extent of movement was selected for each of these subjects, a distance covered by the arm during its shortest swing. Each of the records of these four subjects was then gone over carefully and the time required for the arm to cover this shorter distance was measured. The results as shown by all the data will now be examined in detail.

1. *Lid-Closure Time.* The marked decrease in the time required to induce hypnosis on the four consecutive experimental days can be shown by the means of the average time for eye closure of all the ten subjects. These are found to be 49.72, 25.44, 20.56, and 19.70. It will be seen from the table that on more than one occasion several of the subjects passed into the trance state instantly.

Of considerably greater significance is the question of whether the lids close to trance suggestions any more readily after having responded to the arm suggestions twice than when no such suggestions have recently preceded. The total lid-closure time values for each subject on each of the two types of occasions are given in Table 2. The differences are shown in the third column. There it may be seen that of the ten subjects, seven show a greater or less advantage in trance-induction time following the arm suggestions in conformity to the hypothesis already outlined. Because of a single extremely erratic score on the part of subject G.C., a mean of these values would be without meaning. The median advantage for the trances following the arm suggestions amounts approximately to one second, which is, roughly, 2 per cent of the average trance-induction time. Obviously such evidence is insufficient to give any very considerable support to the hypothesis, though, so far as it goes, it is favorable. Finally, it should be noted that, with a period of 15 minutes of irrelevant activity separating the last arm response from the beginning of the trance suggestion, it is to be expected that any such tendency, not strong at any time, would be fairly weak.

2. *Latent Time.* During the course of the experiment it was observed that nearly all of the subjects exhibited a certain latency of arm movements, i.e., a certain length of time after the suggestions for such movement had begun before any detectable movement of the arm took place. This measure of suggestibility was not possible with the postural technique formerly in use because the point at which response of the subject to suggestion began was always obscured by postural tremors. The arm-cradle technique here employed obviated this difficulty. On the records, the latent time was measured as the interval from the first notch of the signal marker to the first trace of movement of the arm to the left. The means of the latent times for the first and second normal were 16.1 sec. and 8.32 sec., respectively, and for the first and second trance periods, 7.5, and 7.27 sec. These results show the familiar practice effect in that the latent time decreases from the first to the second trial in each state. But our primary concern is with the relative latency in the two states for constant states of practice. In this the 16.1 sec. is contrasted with the 7.5 sec., and the 8.32 with the 7.27. Both stages of practice show, upon the average, distinctly greater responsiveness to suggestion in the trance condition.

3. *Comparative Suggestibility.* When the composite mean of all responses in the normal state is compared with that for the trance state it is found that the trance is about 25 sec. faster, on the average, than the normal state. Trance responses are therefore about 45 per cent faster than those in the normal state. It can also be shown that the average of the first trance responses is 49.4 per cent as long as the average for the first normal responses, while the average of the second trance response is slightly more than 60 per cent as fast as the average of the second normal responses.

A more detailed examination of the data reveals that two subjects only—C.L. and R.G.—were negative in their reactions, i.e., suggestibility for these subjects was decreased in the trance state. All of the other subjects showed more or less increase in suggestibility during the trance. It is extremely difficult to account for these negative cases. It was impossible to experiment upon them further, and, in the light of the other results, it would seem probable that the negative reactions were sampling errors. Present experimentation in hypnosis works under many handicaps, not the least of which is the small number of subjects available for experimental purposes. It is significant, however, that the general tendencies revealed in previous experimentation are substantially verified under

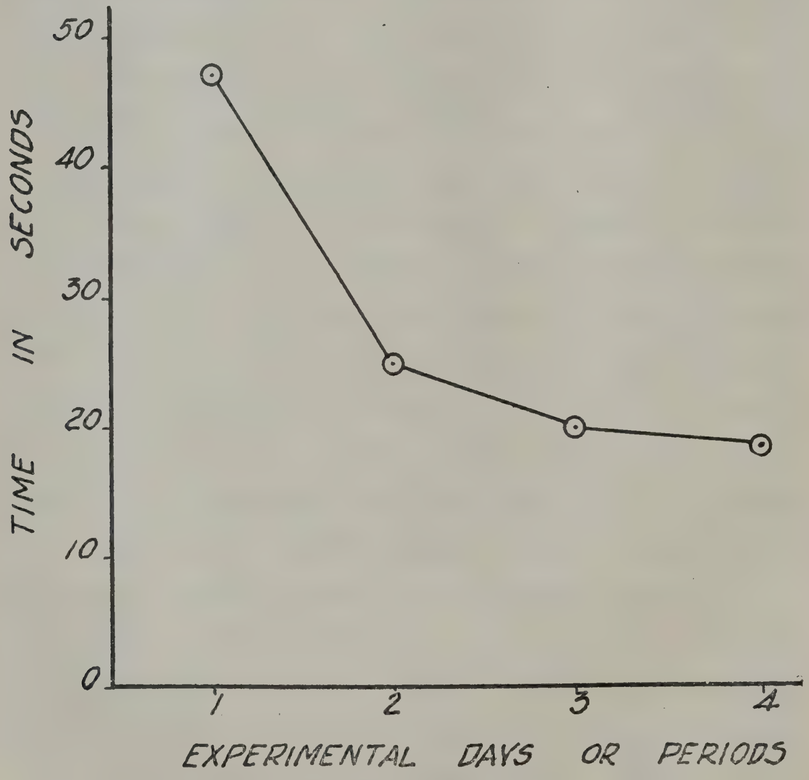

FIGURE 2

Composite Graph Showing the Influence of Repetition on the Rate of Lid Closure at Successive Trance Inductions the more or less changed and, in certain respects, more rigid conditions of the present investigation.

4. *Cumulative Effect.* Comparison of the mean suggestion time of the first normal test with that of the second normal test shows the latter to be about 85 per cent as long. The mean time of response of the first trance suggestion is, however, 1.15 sec. shorter than the second. This mean is unduly influenced by the unusually long response time of subject G.C. This subject evinced quite atypical behavior throughout the experiment.

5. *Perseverative Effect.* It is possible to secure from the data of Table 1 the mean time of response for all trance tests preceding the normal tests. This is 28.62 sec. When this mean is compared with the mean time of response for all trance tests following the normal tests, 28.45, the latter is found to be 99.47 per cent as long as the former. If we now compare the mean of all the normal tests preceding the trance tests, 53.57 sec., with the mean of all the normal tests following the trance, 51.0, we find the latter to be 95.2 per cent as great as the former.

6. *Practice Effect.* Some evidence of a process akin to habit formation seems apparent from an examination of the practice effects. The mean time of the first day's tests is 44.02 sec. When this is compared with the mean time for the tests on the fourth day, 38.97, the saving is found to be 11.48 per cent. Practice effects become more striking when we compare the mean of all the first tests on all the experimental days, 47.0, with the mean of the fourth tests, 35.92. The saving here amounts to over 23 per cent.

It is interesting to note that four of the ten subjects in this experiment spontaneously remarked that during the waking suggestions their arms were pulled to the left by some force external to themselves. These subjects remained quite unconvinced of their error even when, after the experiments were concluded, they were shown that the thread extended to the right and not to the left, and therefore could not be responsible for the reaction.

Summary

The results of the present investigation may be summarized as follows:

1. The facility of trance induction as indicated by the duration of hypnotic suggestion required to produce lid closure is markedly increased by repetition. Between the first and the fourth experimental days the time decreased over 60 per cent.

2. Seven of the ten subjects show a shorter lid-closure time in trance induction where hypnosis is preceded by positive response to waking suggestions than when not so preceded.

3. The latency of response to suggestion averages less in the trance than in the waking state by 34 per cent.

4. Further evidence that the trance is a state of heightened susceptibility to suggestion is contained in the fact that the time required to induce a limited amplitude of movement of the extended arm averages less in the trance than in the waking state by 45 per cent.

5. Cumulative effect is revealed by a comparison of the mean time of response of the first normal with that of the second normal test showing the latter to be about 85 per cent as long. The very atypical behavior of one subject obscures the corresponding tendency in the trance tests.

6. This investigation fails to reveal significant perseverative effect.

7. Practice effect, as indicated by the mean time of all the first days' tests on all the experimental days as compared with the mean of the fourth tests, amounts to a reduction in time of over 23 per cent.

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LA SUGGESTIBILITÉ COMPARATIVE DANS L'ÉTAT HYPNOTIQUE ET L'ÉTAT DU RÉVEIL

(Résumé)

A quatre jours successifs on a testé dix sujets pour la suggestibilité comparative dans l'état hypnotique et l'état non hypnotique. On a testé les sujets deux fois chaque jour dans chaque état avec une période de deux minutes entre les deux tests dans un certain état, et une période de quinze minutes de lecture tranquille entre les tests dans les différents états. La moitié des sujets ont subi les deux tests hypnotiques précédant les deux tests

non hypnotiques le premier jour et le quatrième jour, et les tests de réveil premièrement les autres jours. Les cinq autres sujets ont suivi l'ordre contraire. En outre, la durée de la suggestion hypnotique continue nécessaire pour causer la fermeture de la paupière a été enregistrée au moyen d'une montre à arrêt. La mesure de suggestibilité a été la durée de la suggestion nécessaire pour causer un mouvement de huit pouces du bras droit étendu horizontalement. Le bras a été supporté par une plate-forme qui a pu osciller librement, attachée à un appareil enregistreur qui a tracé les réponses sur du papier fumé. Huit d'entre les dix sujets ont montré une réponse plus rapide à la suggestion du bras dans l'état hypnotique. Le temps nécessaire pour causer la réponse maximale dans l'état hypnotique a été moins que le normal de 45 pour cent. Sept des sujets ont montré un temps un peu réduit pour la fermeture de la paupière sous la suggestion hypnotique, quand l'hypnose a été causée aprés les réponses à la suggestion du bras dans l'état de réveil.

Caster et Baker

DIE RELATIVE SUGGERIERBARKEIT IM HYPNOTISCHEN UND IM WACHENDEN ZUSTAND: EINE WEITERE UNTERSUCHUNG

(Referat)

Es wurden zehn Versuchspersonen an vier aufeinanderfolgenden Tagen in Bezug auf ihre relative Suggerierbarkeit (suggestibility) im hypnotischen und im wachenden Zustande geprüft. Die Versuchspersonen wurden an jedem Tag in jedem Zustand zweimal geprüft. Zwischen den Prüfungen in dem selben Zustande gab es ein Zwischenraum von 2 Minuten, und zwischen den Prüfungen in verscheidenen Zuständen lasen die Versuchspersonen 15 Minuten lang ruhig. Bei einer Hälfte der Versuchspersonen gingen die zwei hynotischen Prüfungen am ersten und am vierten Tag den wachenden Prüfungen voraus, um an den anderen Tagen den wachenden zu folgen. Bei den fünf übrigen Versuchspersonen wurde diese Anordung umgekehrt. Ferner wurde die Dauer der zur Veranlassung der Schliessung der Augen nötigen Suggestion an einer Haltuhr (stop-watch) gemesseen. Als Massstab der Suggerierbarkeit diente die Dauer der Suggestion die nötig war, um eine 8- Zoll-lange Bewegung des horizontal ausgestreckten rechten Armes zu veranlassen. Der Arm wurde von einer frei-schwingenden Ebene (platform) gestützt, die mit einem Registrierungsapparat (recording device) verbunden war, welcher die Reaktionen auf beräuchertem Papier (smoked paper) registrierte. Von den zehn Versuchspersonen erwiesen acht in hypnotischen Zustande eine verschnellerte Reaktion auf die Suggerierung bezüglich des Armes. Die zur Auslösung der maximalen Reaktion nötige Zeit war im hypnotischen Zustande mit 45% geringer als im normalen Zustande. Sieben der Versuchspersonen erwiesen unter hypnotischer Suggerierung eine geringe Herabsetzung der zur Veranlassung der Lidschliessung nötigen Zeit wenn die Hypnose veranlasst wurde nachdem auf Suggerierungen bezüglich des Armes im wachenden Zustand reagiert worden war.

Caster und Baker

A STUDY OF THE RESPONSES OF THREE PSYCHOTIC GROUPS TO A TEST OF SUGGESTIBILITY*

From the Department of Psychology of the University of Rochester

Griffith W. Williams[1]

Introduction

If abnormal and psychotic behavior is to be regarded as a deviation in degree from normal types of behavior, it becomes necessary to carry over problems and methods of research from normal to abnormal subjects. When such methods of research are applied to two or more abnormal groups, a comparative study of these groups becomes possible and the technique used in the research may prove to be of diagnostic value. Such a comparative study is covered by this report. The technique employed has been used extensively by other investigators in work with normal subjects (1, 2, 5) and in this instance is used with three clearly defined psychotic groups.

Statement of Problem

This investigation is an attempt to discover whether or not a test of suggestibility will elicit a characteristic type of response on the part of three psychotic groups diagnosed as (1) dementia praecox, catatonic type, (2) dementia praecox, paranoid type, and (3) manic-depressive, manic phase.

1. One of the distinguishing characteristics of the catatonic type of dementia praecox is negativism. "Any effort to get the patient to do anything is immediately met by a response diametrically opposed

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[1] The writer acknowledges with pleasure the assistance given him by Drs. Waterman and Felt of the Connecticut State Hospital, Middletown, Conn., and Drs. Lonergan and Savitsky of the Manhattan State Hospital, Ward's Island, New York, N. Y. They placed the facilities of their hospitals at his disposal and encouraged his work in every way. It would not have been possible to carry out the investigation without such cooperation.

The writer acknowledges also the assistance of the Institute of Human Relations of Yale University in defraying part of the expense of the work at Ward's Island.

to the desired act" (4, p. 207). This is usually tested by such acts as opening the mouth, opening and closing the eyes, etc. The problem undertaken here is to discover whether it is possible to record this negativistic type of response in a way so that it can be objectively measured and evaluated. A further problem is to find out what occurs when a test used to determine normal suggestibility is given to this type of patient.

2. The paranoid type of dementia praecox usually shows a somewhat loosely organized delusional system with some hallucinations (4, p. 212). The delusions frequently refer to persecution or ideas of reference and influence (3, p. 274). A study of the response of this group to a test of suggestibility becomes significant in view of their resentment to being "hypnotized," "under mental control," etc. This test constitutes a direct attempt to influence their behavior. What will be their response to such an attempt? Will they show a negative or a positive response? If positive, will this indicate an excessive suggestibility on their part which may be a determining factor in their psychosis?

3. In the manic phase of the manic-depressive psychosis, "distractibility is a prominent feature and the almost constant flow of ideas frequently refers to something seen or heard in the environment" (4, p. 146). In this condition, how will the subjects respond to definite changes in the environment brought about in a deliberate attempt to influence their behavior? Is there a close association between distractibility and suggestibility? Is their motor behavior modified by factors in the environment in the same way as their verbal behavior?

The problem, then, becomes one of applying a test of suggestibility to three psychotic groups, each of which shows a type of pathological behavior which is likely to evoke a definite response to the test.

Apparatus and Procedure

Both the apparatus and the procedure are identical in all essentials with those already described by the writer (5, pp. 13, 16). Only a brief summary will be given here. A tracer was attached to the subject's clothing at the level of the shoulders. This was done without the subject's knowledge. Any forward movements of the subject were recorded as an upward movement and backward movements as a downward movement of the tracer on the kymograph record.

The subject was blindfolded and then stimulated by having the experimenter suggest to him that he was falling forward.[2] These suggestions were continued for three minutes unless the subject had made a definite response before the end of that time. Two records, separated by an interval of at least three minutes, were made on each subject. During the interval between the records the subject was sent from the room so as to secure a definite break in the continuity of the experiment. Nothing was said to the subject concerning the purpose or nature of the experiment. Either a nurse or attendant of the same sex as the subject sat in the room throughout the experiment except for a few of the paranoid dementia praecox cases.

The method adopted in scoring the records was to draw a line through the record of the half-minute preceding the beginning of the suggestions and another through the last half-minute of the suggestions. A deviation of less than 1/4 of an inch in either direction from the level of the half-minute preceding the suggestions was regarded as indicating no response to the stimulation. The movement of the subject corresponding to this 1/4 of an inch deviation would be 3/4 of an inch, owing to the way in which the movement is reduced by the pulleys on the tracer. In general, the result of adopting this criterion is to increase the number of "no response" reactions. In view of the actual results obtained, this effect tends to emphasize the general conclusions. A record showing more movement than this minimum can then be clearly identified as a positive or negative response.

SUBJECTS

The data were obtained from a group of 83 subjects divided as follows:

32 men, dementia praecox, catatonic type
21 women, dementia praecox, catatonic type
12 women, dementia praecox, paranoid type
18 women, manic-depressive, manic phase

The accuracy of the diagnosis of such a group is a problem which will arise in all work of this nature. No method can reduce all the variables involved to scientific control. In order to minimize this difficulty, however, all cases on which there was any question of diagnosis were excluded. In view of the extreme care with which most of these cases were diagnosed and this diagnosis subsequently

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[2] The content and method of giving the suggestions are fully described in Williams (5, p. 16).

TABLE 1

SHOWING, WITH REFERENCE TO THE PERIOD OF UNCHANGED DIAGNOSIS, THE RANGE, THE AVERAGE, AND THE SHORTEST PERIOD REPRESENTED IN THE 50% OF THE CASES WITH THE LONGEST TERMS OF UNCHANGED DIAGNOSIS

	Range					Average		Longest 50%	
Group	Yrs.	Mo.	to	Yrs.	Mo.	Yrs.	Mo.	Yrs.	Mo.
D.P., catatonic	21	5	—	0	1	4	3	5	0
D.P., paranoid	16	10	—	0	4	4	6	5	8
M.-D., manic	6	0	—	0	1	0	10½	0	5

rechecked, it can safely be said that the accuracy of the diagnosis represents good, if not superior, hospital practice in this respect.

Table 1 shows the data relative to the length of time these cases had maintained an unchanged diagnosis in the group in which they were classified for this experiment.

The range, average, and lower limit of the longest 50% of the manic-depressive cases are considerably less than those of the other groups. This is primarily due to the nature of the disease and does not necessarily cast doubt on the accuracy of the diagnosis. Cases were used with a diagnosis of about one month but who had been having repeated manic attacks for a period of over fifteen years. Furthermore, some of the catatonics with a short period of unchanged diagnosis had been under observation at the hospital for from one to four years before the diagnosis was definitely made.

DATA AND RESULTS

In Figure 1 six records are reproduced showing the three most characteristic types of response. There is a marked difference in the way a positive and a negative response is shown on the records. This is due to the fact that in the positive response the tendency is to swing the body forward onto the toes. In this way a considerable movement of the body can occur without actually losing balance. In the backward movement of the body which is characteristic of a negative response, it is possible to sway the body to only a very limited degree without losing balance. In this case the subjects either shuffled backwards in a steady movement or leaned backwards as far as possible and then made a sharp adjustment of the feet. A record of both types of response is included in Figure 1.

In Table 2 the total number of responses obtained from each group is given together with the number and percentage of this

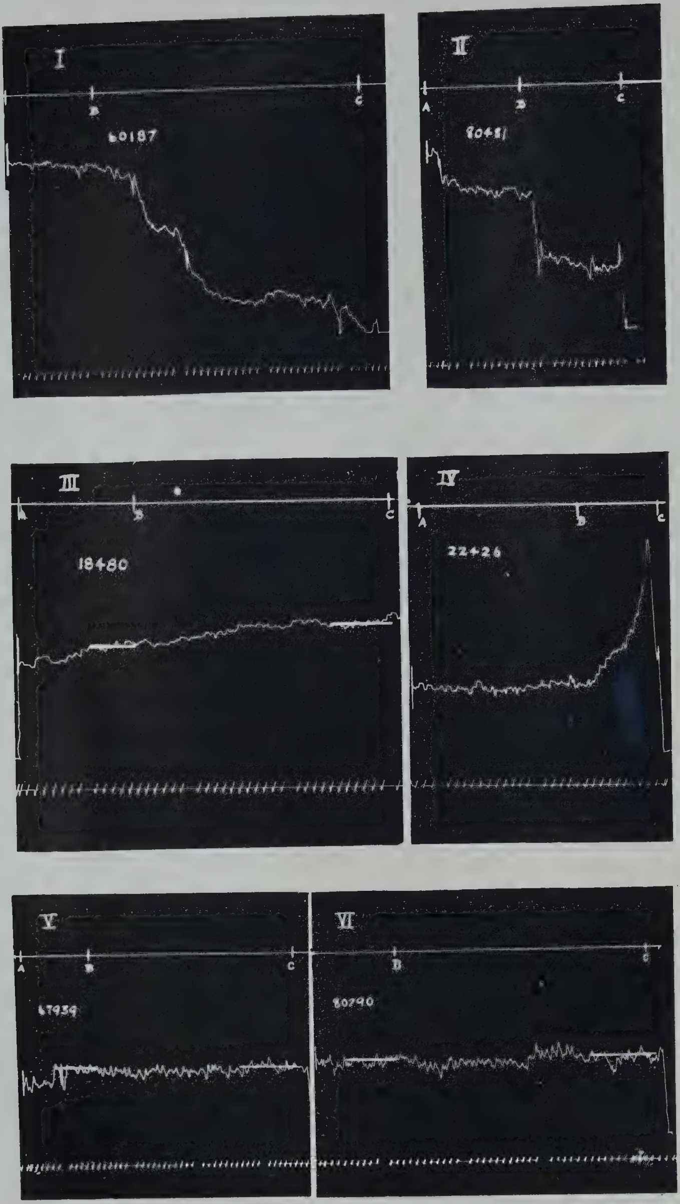
I
B
C
60187
II
A
B
C
80481
III
A
B
C
18480
IV
A
B
C
22426
V
A
B
C
67939
VI
B
C
80790

FIGURE 1

FIGURE 1

Showing Six Records of the Most Typical Responses Made by the Experimental Group

Each record has (1) a signal line marked *A* where the experimental period begins, *B* when suggestions are given, and *C* at the end of the suggestions; (2) the file number of the subject in hospital; (3) a record of the response, and (4) a time line marked in 5-sec. intervals.

Records I and II are typical dementia praecox catatonic negative responses. No. I is typical of the slow, backward, shuffling movement, while No. II shows two sharp, distinct, backward movements of the feet.

Records III and IV are typical positive responses of the dementia praecox paranoid group. No. III shows the lines used in scoring and indicates a slight response, but clearly positive. No. IV shows a response which results in the subject falling forward until stopped by the experimenter.

Records V and VI are typical of the lack of response shown by the manic-depressive manic group. The lines used in scoring are shown in these records also. It is obvious that similar lines are not required on the other records.

total showing (1) a positive response, (2) no response, and (3) a negative response. Two responses were secured from each subject.

The most obvious feature of these data is the difference in the responses of the two dementia praecox groups. Negative responses occur most frequently with the catatonic group while positive responses occur with a similar frequency in the records of the paranoid group. The negative responses shown in Figure 1 indicate that there is an active rather than a passive response to stimulation. The active nature of the response is emphasized by the fact that some of the catatonics kept repeating "backwards" in a faintly audible voice during the entire period of stimulation by the experimenter. The problems raised in connection with the catatonic group can be answered by saying that the characteristic response of a catatonic to a test of suggestibility is a negative one and that the technique of this test provides a simple and objective means of recording such responses.

TABLE 2

Showing the Total Number of Responses Obtained from Each Experimental Group Together with the Number and Percentage of Each Type of Response Included in This Total

Group	Total number of responses	Pos. resp.		No resp.		Neg. resp.	
		No.	Per cent	No.	Per cent	No.	Per cent
D.P., catatonic	106	11	10.3	32	30.3	63	59.3
D.P., paranoid	24	13	54.1	11	45.9	0	0.0
M.-D., manic	36	10	27.7	21	58.3	5	13.9

The data on the history and diagnosis of the catatonic cases do not give any clue for the explanation of the positive responses secured from eight members of this group. Three subjects gave a positive response on each trial. The other five positive responses were coupled with either a negative response or no response. Six other subjects show a change in the type of response given in the two records, either from a negative to no response or vice versa.

The presence of a large number of records showing no response in the paranoid group makes these data somewhat more difficult to interpret. The records show an almost equally strong tendency to respond positively or to remain without giving any response. The total lack of negative responses is probably significant. There is a tendency to uniformity of response on the part of the individual subjects, as only three of the twelve subjects showed any change in response between the two records. The questions asked in stating the problem associated with this group cannot be clearly answered. It is significant, however, that approximately one-half of the group respond positively when they resent being "influenced" by other people. Ordinarily a definite negative response would be expected. The group tested seem to show a tendency to be suggestible, though the data do not warrant a statement of the possible relationship of this suggestibility to the nature of their psychosis.

A tendency not to respond to direct suggestions made by another person is fairly characteristic of the manic group. There is, however, a lack of uniformity of response on the part of individual subjects, as one-half of the subjects changed their responses given on the first record when they were tested the second time. Furthermore, no subject gave two negative responses. The questions raised in connection with this group can be answered fairly definitely within the limits of this investigation. In spite of their well-known recognition of slight changes in the environment and their responses to these changes, this group shows a strong tendency not to respond when a deliberate attempt is made to influence their behavior. The results also tend to indicate a clearly defined difference between the type of suggestible response elicited by this test and the "distractibility" which is very characteristic of this group. It is possible, though not proved by these data, that the distractibility is restricted to verbal behavior and does not include changes in motor behavior.

A problem raised by these results, but not answered within the scope of this experiment, concerns the nature of the behavior designated "no response." A large percentage of the entire group shows this form of behavior. Further research on this problem as it occurs among normal subjects will be necessary before its significance for an abnormal group can be evaluated.

Conclusions

1. This technique provides a means of objectively recording the negativistic responses of catatonics on a test used to demonstrate normal suggestibility.
2. The catatonic and paranoid forms of dementia praecox are distinguishable by the negative suggestibility of the former and the tendency to positive suggestibility on the part of the latter.
3. The manic phase of the manic-depressive psychosis is marked by a lack of responsiveness to outside stimulation such as is used in this technique.
4. The tendency to distractibility on the part of the manic subjects does not seem to be directly related to suggestibility as indicated by this test.
5. The presence of a large number of records showing no response to a test of suggestibility raises the question of the nature of this behavior and its relation to positive and negative suggestibility.

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UNE ÉTUDE DES RÉPONSES DE TROIS GROUPES PSYCHOTIQUES À UN TEST DE SUGGESTIBILITÉ

(Résumé)

Un test de suggestibilité qu'on a employé pour des personnes normales est employé dans cinquante-trois cas de Démence précoce, type catatonique; dans douze cas de Démence précoce, type paranoïde; et dans dix-huit cas de Psychose maniaque dépressive, phase maniaque. Dans le test on donne des suggestions au sujet dont les yeux sont bandés, qu'il tombe en avant et les mouvements de son corps sont enregistrés par un indicateur. Les réponses sont divisées en celles qui montrent la suggestibilité positive, le négatisme, et celles où il ne donne aucune réponse à la stimulation. Cinquante-neuf pour cent du groupe catatonique a donné des réponses négatives, cinquante-quatre pour cent du groupe paranoïde a donné des réponses positives, et cinquante-huit pour cent des résultats du groupe maniaque n'a montré aucune réponse à la stimulation.

Les résultats obtenus montrent que (1) la méthode employée le rend possible de procurer des résultats objectifs des réponses catatoniques négatives; (2) le groupe paranoïde montre une tendance marquée à la suggestion positive et le groupe catatonique montre une tendance marquée à la suggestion négative; (3) les maniaques montrent une tendance à ne pas répondre à la stimulation extérieure malgré leur tendance marquée à la distraction, un résultat qui rend plus facile la distinction entre la suggestibilité et la tendance à la distraction.

WILLIAMS

EINE UNTERSUCHUNG DER REAKTIONEN VON DREI GRUPPEN GEISTIG ABNORMER INDIVIDUEN (PSYCHOTIC GROUPS) AUF EINE PRÜFUNG DER SUGGERIERBARKEIT (SUGGESTIBILITY)

(Referat)

Es wird eine Prüfung der Suggerierbarkeit, die schon an normalen Individuen gebraucht worden ist, auf 53 Fällen von Dementia Praecox, katatonischer Art; 12 Fällen von Dementia Praecox, paranoider Art; und 18 Fällen manisch-depressiven Irreseins in manischem Zustande angewendet. In der Prüfung wird der Versuchsperson, deren Augen verbunden sind, suggeriert, dass sie forwärts fällt (is falling), und es werden ihre Bewegungen graphisch registriert (recorded by a tracer). Die Reaktionen werden gesondert in die, die positive Suggerierbarkeit erweisen, die, die Negativismus erweisen, und die, in denen auf den Reiz keine Erwiderung gemacht wird. Bei der Gruppe der Katatoniker waren 59% der Reaktionen negativistisch, bei der Gruppe der Paranoiker waren 54% der Reaktionen positiv, und in der Gruppe der Manischer gab es in 58% der Fälle keine Reaktion auf den Reiz.

Die Befunde erweisen, (1) dass die verwendete Methode eine objektive Registrierung negativistischer katatonischer Reaktionen ermöglicht; (2) eine ausgeprägte Neigung der paranoiden Gruppe in die Richtung der positiven Suggerierbarkeit, und der Katatoniker in die Richtung der negativen Suggerierbarkeit; und (3) dass die Manischen dazu neigen, trotz ihrer ausgeprägten Ablenkbarkeit (distractibility) auf äussere Reize nicht zu reagieren, ein Befund, der die Unterscheidung zwischen Suggerierbarkeit und Ablenkbarkeit erleichtert.

WILLIAMS

EXPERIMENTAL STUDIES IN OCULAR BEHAVIOR: I. THE DODGE MIRROR-RECORDER FOR PHOTOGRAPHING EYE-MOVEMENTS*[1]

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Introduction

This series of studies is concerned with (1) the adaptation and refinement of experimental instruments and methods for an intensive and extensive photographic registration of eye-movements, (2) the normal and abnormal aspects of ocular behavior when the eye pursues moving objects and when eye-movements are initiated voluntarily, and (3) the determination of the relationships between eye-movement characteristics and higher mental activity such as performance on intelligence and placement tests. The present paper reports experimental data on the Dodge mirror-recorder when applied to a large number of subjects with voluntarily initiated eye-movements.

Three considerations are fundamental in studying the various phases of eye-movements. In the first place, the rôle of eye-movements in behavior and adjustment of the organism has widespread biological significance. Ocular behavior maintains a dominant position in the major portion of our adaptive behavior, e.g., our contacts with each other, our multifarious reactions to a complex environment, and in acquiring knowledge of the world, people, and things. Eye-movements, in particular, are of very great importance in the whole chain of events involved in reading, in assuming and maintaining postures, in restoring and maintaining equilibrium of our bodies with

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[1] The author gratefully acknowledges the financial assistance granted him by the Committee on Grants-in-Aid of the National Research Council for partial defrayment of expenditures incurred in this series of experimental inquiries.

The foundation for this series of experimental studies of ocular behavior was laid in the laboratories of the Institute of Human Relations, Yale University, while the writer was a National Research Council Fellow under the supervision of Professor Raymond Dodge, whose insight into psychological problems and courteous human sympathy will always be an inspiration.

respect to gravity, in appreciation of the beautiful, and in constructive effort.

In the second place, instruments and techniques for the objective registration of eye-movements have been developed to the stage where considerable scientific accuracy is obtained.

The third important consideration is that the various phases of eye-movements afford unique points of departure in determining the relative influences of different levels of neural integration in the initiation, control, and inhibition of response. Evidence supporting the latter statement may be found in the reports of Dodge (4) of his various studies of human variability and by the writer in his study (14) of the reciprocal effects of ocular pursuit and the vestibular reflex which influence adaptive behavior. Further problems have to do with the neural relationships in the voluntary and reflex control of human adjustment as development proceeds from childhood to adult maturity, with special reference to the interrelationships of neuro-muscular and intellectual processes. To study the latter problem would mean to observe and measure through controlled experimental conditions the maturation of the processes of inhibition and the control of the higher over the lower neural centers in children and adults. Results previously obtained by Dodge and the writer and data to be presented in forthcoming reports indicate that eye-movement patterns are quite stable in adults, that the general characteristics change very little during practice and show two general types, the reflex and the voluntary, which can be distinguished experimentally. If we were to find, for example, that an individual under experimental conditions shows a lack of control of the higher over the lower neural centers as may be manifested in the two types of eye-movements, then we might ask: Does the same individual show a lack of control in intellectual and social adjustments? Other important questions arise: When and how do these two types of eye-movements develop in the child? How are they related to the development and persistence of reading habits? These to me are fundamental questions in relating basic neuro-muscular patterns to the adequacy of intellectual and social adjustment.

It is our purpose in the first report of this series to present experimental data indicating the validity and feasibility of the use of the Dodge mirror-recorder for a large number of subjects in studying the speed, latency, and characteristics of the pursuit and saccadic phases of eye-movements. This has been a rather laborious task, but one I consider worth while from the standpoint of determining the adequacy of experimental techniques.

Previous Work

Space will not permit us adequately to survey the literature on the various methods of recording eye-movements. However, some of the most recent and pertinent studies must be considered.

Dodge (2, 3) has described apparatus and techniques for recording the five types of eye-movements, namely, the saccadic, pursuit, coordinate compensatory, reactive compensatory, convergence and divergence with the corneal reflection method and with his mirror-recorder lightly applied to the closed eyelid. Tinker (13) has surveyed the literature and successfully applied an adaptation of the Dodge corneal reflection method to studies of eye-movements in reading (11, 12). Miles and Shen (10) adapted the Dodge corneal reflection method to a study of eye-movements in the reading of Chinese in the vertical and horizontal planes. Vernon (17) described various methods used up to 1928, and has recently reported his corneal reflection method and apparatus (18) for photographically recording eye-movements in reading. McGinnis (9) studied the initiation and development of eye-movement patterns in the infant with the motion-picture method and included in this recent report a brief survey of the methods of recording eye-movements in children and adults. He has also included an excellent bibliography of sources to the literature on studies of ocular behavior in reading and nystagmus.

Dodge and Fox (6, 7) and Dodge, Travis, and Fox (8) have recently applied the mirror-recorder method in studies of ocular behavior of patients suffering from brain tumors and involvements of the visual and oculo-motor pathways. Travis (14) studied the reciprocal effects of visual and vestibular stimulation on eye-movements with the Dodge mirror-recorder method. Another study by Travis and Dodge (16) employed the same method in determining the effect of temporarily disappearing objects on ocular pursuit.

The Essentials of the Experimental Instruments

Some repetition of former description will be necessary to present adequately the experimental technique, method, and apparatus for eliciting and photographically recording the pursuit and saccadic phases of eye-movements with which we are concerned in this series of studies.

The mirror-recorder for the closed eyelid is the central part of the apparatus, and has been described in detail by Dodge (3) who developed it as early as 1916, and this method has been used in a number of studies by Dodge and, more recently, by the writer. The essential features of the Dodge mirror-recorder will be presented in view of the fact that the entire experimental set-up is dynamically related to it.

In the words of Professor Dodge (3, p. 171), "the mirror-recorder was designed to meet the following requirements:

> "1. A rigid frame which can be attached to any shaped head and will not vibrate when the head moves. This must be so made that the adjustable attachments for the recording mirrors will interfere least with the supports for the head and other experimental attachments.
>
> "2. Attachments to this frame must hold the mirrors against the eyelids with gentle and even pressure. They must be adjustable horizontally to interocular distance; vertically, to the horizontal meridian of the eye. Secondary adjustments must permit the direction of the recording beam to the slit of the recording camera.
>
> "3. The recording mirrors should project a sharply defined image of suitable shape and dimensions. They must be so mounted on the arms that hold them, that they are free to rotate in conformity to the position of the cornea. At least, they must rotate freely on an axis that is parallel to the axis of the eye-movements which they are to record."

The frame of the mirror-recorder may be constructed from brass or aluminum tubing (5 mm. outside diameter), with an adjustable nose-piece made of hard rubber shaped to rest on the bridge of the nose, and adjustable temple tubes, in and out of which slide the bars which ride over the ears. Adjustable secondary side supports of brass tipped with hard rubber are attached at right angles to the temple-tubes and press against the head above the temples. This five-point suspension affords rigidity and eliminates accidental vibration.

The nose-piece-tube extends about 2 cm. beyond the point of attachment of the temple tubes to allow for adjustment to various differences in head width. This extension of the nose-piece-tube receives the mirror-arm which carries the mirror-recorder attachments proper. The mirror-arm slides smoothly in or out of the nose-piece-tube, thus moving the mirror-recorder nasalward or templeward as experimental conditions may demand.

A concave mirror about 4 mm. square is mounted on a block of wood (5 mm. by 5 mm. by 10 mm.) which is pivoted in the center to a very light forked arm which, in turn, is fastened at right angles to a short upright rod (20 mm. long by 2 mm. in diameter). The upright rod is mounted to a hard rubber base by adjustable cone bearings. This hard rubber base carrying the cone bearings, the forked arm, and the mirror-block can be adjusted very smoothly up or down, or turned on a vertical axis on the mirror-arm which, as stated above, slides smoothly in and out of the nose-piece-tube.

The axis of the mirror block is parallel to the axis of horizontal rotation of the eye, so that when the eye rotates clockwise the mirror-block rotates counterclockwise. The relationships of the mirror-recorder proper to the main frame afford the greatest flexibility in adjustment of the mirror-block so it will rest under all conditions of head size and shape, with a light constant pressure on the closed eyelid. When all the adjustments are made, this light, constant pressure is produced by the tension of a small rubber band which is stretched between the forked arm carrying the mirror-block and the mirror-recorder base. The tension of the rubber band may be made greater or less by turning the mirror-recorder base on its vertical axis. This is a very important adjustment, because it controls the amount of pressure imposed by the mirror-block on the closed eyelid. If the pressure is too great, the subject experiences discomfort; if insufficient pressure is imposed the mirror-block will not make adequate contact with the eyelid, thus producing distortions in the record.

The lid of the recording eye was held firmly but comfortably closed by a specially designed lid-holder mounted on the temple-tube and was adjustable in both horizontal and vertical planes. When in place the lid-holder held the eye lashes of the upper lid well over the lower lid against the skin over the orbital edge of the superior maxillary bone. When properly placed the lid-holder diminished lid tremors to a minimum and eliminated the tension associated with the subject's attempts to voluntarily hold one eye closed.

In using the mirror-recorder on a large number of subjects, the matter of adjustment of the various parts may become time-consuming. The temple-tubes, if adjusted slightly wider than the average width of head, may serve for a large majority of subjects.

Under these conditions the recorder frame is made firm without discomfort by pressing the secondary side supports firmly against the head and by adjusting the riding temple bars behind the ears. The subject is then brought into position with the back of his head resting comfortably against the headrest, with his upper teeth resting gently on the adjustable mouth-bit. The lid-holder which is mounted on the temple-tube is adjusted to hold the lid closed without discomfort. The mirror-arm is then adjusted to bring the eye-recording mirror-block in contact with the lid at such a position and angle that it rests as accurately as possible over the apex of the cornea, at the same time reflecting the recording beam across the slit of the camera. The concave mirror of the nose-piece is then adjusted to record head movements. These various adjustments seem detailed and complex, but with some practice they are made quickly and accurately.

The source of illumination was a plain 120-watt horseshoe filament electric bulb mounted in a black tin box with a narrow slit above and slightly in front of the subject, at such a distance to produce a sharp image of the filament when reflected from the concave recording mirrors to the horizontal slit of the camera 58 cm. from the eye. This distance allowed sufficient magnification for our purposes.

A light-proof tube, 75 cm. long and 15 cm. square at proximal end, and 42 cm. by 15 cm. at distal end, served to eliminate extraneous stimuli. The subject fixated the moving stimuli and fixation points at the distal end of the tube with the right eye, while the conjugate movements of the closed left eye were being recorded. A dark cardboard background was mounted about 10 cm. beyond the distal end of the light tube; the fixation points and the stimuli were presented between this background and the distal end of the tube. The stimuli and background were illuminated by a long showcase electric bulb mounted in such a way just inside and at the top of the distal end of the light tube so that none of the rays from the light fell directly on the subject's eye.

The camera was driven through a small rope belt by a Pacent electric phonograph motor both of which were mounted to the light tube. The speed of the camera was varied by the friction adjustment on the motor and by cone pulleys. The present form of the continuous photographic recording camera was designed and constructed by Dodge and the writer at Yale. The roll of sensitized paper (200 ft. long, 4 in. wide) and the rollers are enclosed in a wooden box (35 cm. by 18 cm. by 12 cm., inside measurement), one side of which can be easily and quickly removed in the dark room to remove exposed paper or to replace new unexposed paper. A large wooden roller (7 cm. in diameter) and a small driving brass roller (2 cm. in diameter), the latter wrapped smoothly with one layer of friction tape, are held together by coiled springs mounted externally on each side of the camera box. The rollers are mounted within a thin galvanized steel frame which fits inside the wooden box. This steel frame prevents distortion in alignment of the rollers in case of shrinkage of the wooden box. The paper is moved by friction as it passes between the two rollers, and thus runs at a constant predetermined speed. As the paper unwinds over the large roller it passes the horizontal slit (1 mm. wide) in front of which is mounted a cylindrical lens to focus a sharp image of the light filament onto the sensitized paper, which is moved approximately 7.5 cm. per second.

The stimuli for elicitation of pursuit movements may be moving black bars, numbers, letters, words, or objects (depending on the nature of the problem) mounted on a chain which runs at uniform speed in either direction across the distal end of the light tube. The chain runs over pulleys and is driven by an adjustable-speed electric motor. Neither the chain nor the wooden stimuli attachments are visible to the subject. In our experiments, this chain which carries the stimuli was 86 cm. from the eye of the subject. Time was measured in seconds and 1/100 second through circuits from a seconds-pendulum and a 100 d. v. electrically driven tuning fork.

I have found the uniformly moving chain a very suitable arrangement for the presentation of moving visual stimuli in the elicitation of pursuit movements and optic nystagmus.

A sturdy chair with backrest, headrest, and mouth-bit attachment was solidly mounted to what is practically a duplication of the Dodge rotation platform (16). The light tube, camera and motor, and chain belt for moving stimuli are supported at center of rotation of the platform just above the subject's head. The distal end of the tube and accessary apparatus are further supported by guy wires extending to the top of the tripod support of the rotation platform.

This arrangement of the eye-movement apparatus in conjunction with the apparatus for studying bodily motion affords a useful set-up to study conjointly the inhibiting and re-enforcing effects of stimuli in the visual and vestibular systems.

EXPERIMENTAL RESULTS

As Dodge (3) has previously stated, "a mirror which is pressed lightly against the lid over the cornea will always tend to assume a position tangential to the surface of the cornea on which it rests as far as this is permitted by its mounting. . . . The angular displacement of the mirror will depend on the following factors: (*a*) The angular displacement of the eye. (*b*) The geometrical relationship between the radius of curvature of the cornea and that of the eyeball. The greater the differences in curvature the greater will be the movement of the mirror. (*c*) The relative position of the mirror with respect to the apex of the cornea. The greatest conceivable angular rotation of the mirror would result if the apex of a cornea of zero radius of curvature, infinitely eccentric to the globe of the eye, just passed the axis of the recording mirror. On the contrary, no movement of the mirror would result if it rested on a spherical eyeball while the latter rotated on its geometrical center. (*d*) The thickness and stiffness of the intervening lid. If the lid were totally unyielding all movement of the mirror would cease. If the lid conformed absolutely to the underlying surface it would operate on the geometrical relations as an increased radius of curvature of the cornea. (*e*) The intercurrent movements of the lid. Unless the lid is prevented from moving, voluntarily or by a lid-holder, all lid-movements will operate to move the mirror as though the eye moved."

An additional factor in connection with the angular displacement of the mirror is, namely, how can we be certain in the case of our experiments herein reported, with one eye open moving voluntarily and in accordance with moving stimuli, that the closed recording eye is moving likewise? In other words, under our experimental conditions, do the two eyes move conjugately? In our experiments the convergent and divergent movements were reduced to a minimum by maintaining a constant distance between the stimulating field and the seeing eyes; although, under these conditions, we are never sure of a direct control in this respect. Whether the closed recording eye rotates identically in terms of direction and extent with the open seeing eye presents the problematic basis for this experimental inquiry.

The determination of the relationship between the amount of deflection of the beam of light from the concave mirror when the mirror-block is properly adjusted to the closed eyelid and the actual amount of rotation of the seeing eye is of fundamental importance in the validation of the Dodge mirror-recorder as an instrument for the photographic registration of eye-movements.

The type of eye-movement recorded to determine this relationship is of importance. Under the conditions, the use of one eye as the recording eye, the other eye remaining open and fixated on a slowly oscillating pendulum, would be perhaps the most ideal condition for determining this relationship; whereas voluntarily refixating definitely spaced fixation points equidistant from the eye would be the crucial test of the validity of the recorder. Under the latter conditions the eye is very rapidly accelerated from a state of rest to the most rapid speed, and very rapidly decelerated from this high speed to the stationary condition. The question is, how accurately do the deflections of the mirror-recorder correspond in direction and amount with the actual direction and extent of movement of the eyeball?

The voluntary refixation of points spaced at 5, 10, 15, and 20 degrees of the visual field with the right eye, while the mirror-recorder rested lightly on the closed lid of the left eye, afforded conditions to validate the recorder for saccadic movements. The amount of magnification is indicated by the halftone of an actual record in Figure 1, which shows the amount of deflection of the mirror-recorder while the subject refixated points spaced at 0.5, 1, 2, 3, 4, 5, 10, and 15 degrees of the visual field.

An eye-movement of 0.5 degree is not observable by the naked eye, yet movements of this order, when recorded on photographic paper with the mirror-recorder, have an amplitude of about 7 mm. with the magnification used in this study. The extent of these deflections shown in Figure 1 on the sensitized paper was plotted against the actual distances between refixation points and the relationship is represented by the heavy line in Figure 2, which shows a tendency towards rectilinearity. Figure 1 and the data plotted in Figure 2 represent a typical record and data from one subject.

Figure 3 represents distributions of deflections of the beam of light (in mm.) from the mirror-recorder for 54 refixations of points at 5, 10, 15, and 20 degrees of the visual field for Subject B. The distributions are fairly normal, although the measures are closely grouped around a central tendency. The broken line running through the means of the distributions represents practically a rectilinear relationship between the extent of deflection of the mirror-recorder and the amount of rotation of the eyeball.

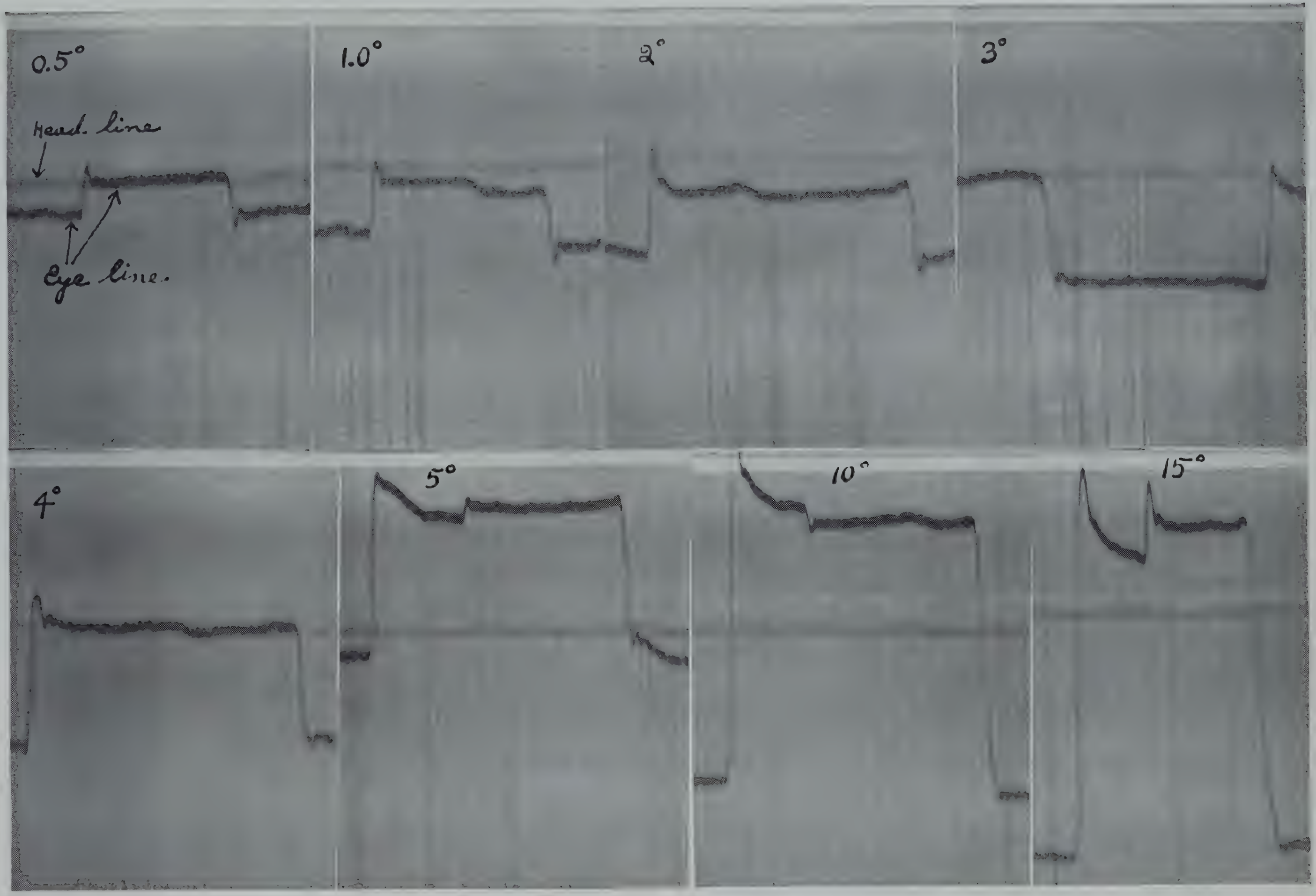

FIGURE 1

Photographic records of conjugate movements of left closed eye with Dodge mirror-recorder when right eye refixated points spaced at 0.5, 1, 2, 3, 4, 5, 10, and 15 degrees, respectively, of the visual field. A clockwise and a counterclockwise movement for each distance is shown. Subject A.

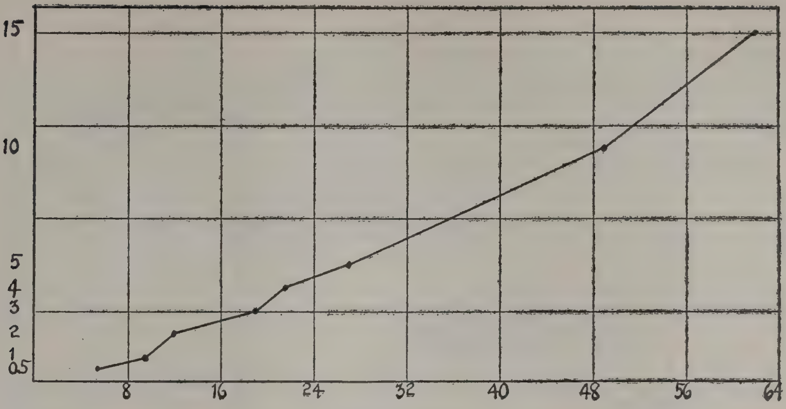

FIGURE 2

Relationship between amplitude of beam of light on photographic record and amount of rotation of the eyeball. Data taken from Figure 1. Angular distance in degrees between refixations represented on the ordinate. Amplitude in mm. of photographic record for each angular distance represented on the abscissa. Subject A.

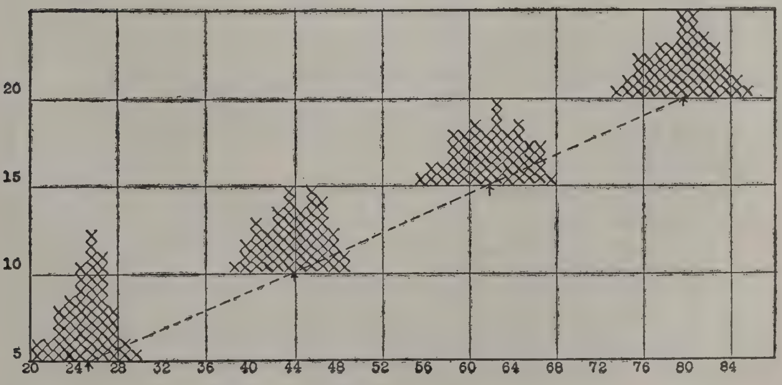

FIGURE 3

Same relationship as in Figure 2, except the distributions of the amplitudes for successive refixations are plotted for each angular distance of visual field, indicating in addition to Figure 2 the variability between 54 successive refixations. Subject B.

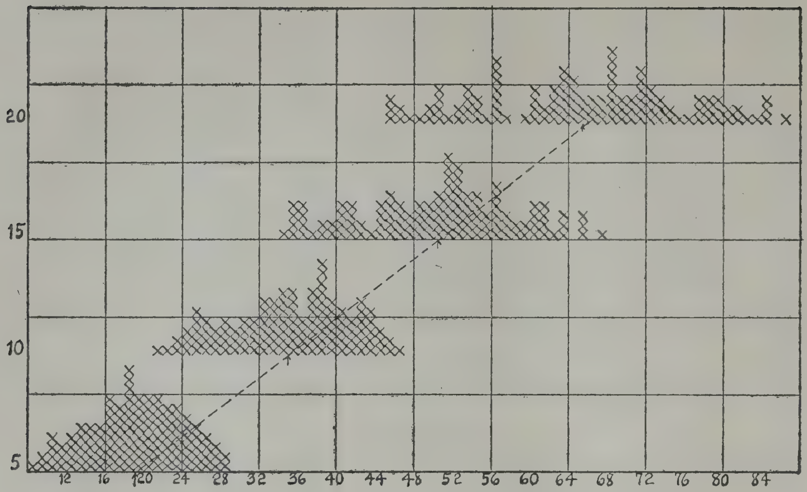

FIGURE 4

Same relationship as in Figure 2, except the distributions of the average amplitudes for 113 individuals are plotted for each angular distance of the visual field, indicating in addition to Figure 2 variability between individuals.

Figure 3 is typical of the variability in successive refixations for one subject. The relative contributions of faulty fixations, lack of contiguity in movements of the two eyes, and errors in the operation of the mirror-recorder to this variability cannot be ascertained, but previous experiments on visual fixation (2) point towards the hypothesis that much of this variability can be accounted for on the basis of faulty fixations.

To determine the practical usefulness of the mirror-recorder for many different subjects data were obtained from 113 subjects (Table 1) under conditions similar to those represented in Figure 3. The means for all subjects are represented by the distributions in Figure 4, and the broken line drawn through the means of these distributions again confirms the rectilinearity in relationship between the extent of the deflection of the beam of light from the mirror-recorder and the amount of rotation of the eyeball, in refixating the definitely spaced points in the visual field. Furthermore, a line drawn through the means for each subject in Table 1 shows practically the same sort of rectilinear relationship, although the slope of this line

TABLE 1
Extent of the recorded beam of light from the Dodge mirror-recorder when the subject refixated points at 5, 10, 15, and 20 degrees of the visual field. Each figure represents the average of approximately 20 refixations.

	Extent (in mm.)					Extent (in mm.)			
Cases	5°	10°	15°	20°	Cases	5°	10°	15°	20°
1	25	43	61	79	57	15	33	50	65
2	12	26	35	45	58	21	37	52	68
3	19	38	57	77	59	17	33	48	62
4	9	28	48	63	60	24	43	60	78
5	10	25	38	50	61	14	27	40	53
6	14	32	45	56	62	21	41	60	80
7	27	38	50	63	63	14	27	41	54
8	20	36	44	56	64	22	42	59	74
9	18	38	63	78	65	22	42	53	62
10	20	35	67	84	66	15	31	46	62
11	23	43	65	83	67	25	43	60	76
12	15	35	49	66	68	21	42	61	81
13	10	28	44	59	69	14	25	36	47
14	25	44	62	81	70	24	42	58	72
15	8	21	35	48	71	23	42	61	79
16	12	26	38	51	72	17	31	45	61
17	18	36	52	66	73	21	41	60	80
18	17	34	51	68	74	20	41	53	71
19	24	42	56	72	75	16	31	45	60
20	21	34	47	60	76	21	41	59	78
21	9	22	34	45	77	18	36	51	69
22	10	23	39	53	78	22	40	56	71
23	17	29	40	52	79	18	36	54	72
24	11	23	36	49	80	24	40	57	73
25	13	29	43	55	81	22	38	53	70
26	10	24	36	46	82	16	32	49	68
27	19	33	48	57	83	26	40	53	67
28	16	29	42	54	84	22	38	54	70
29	18	33	47	60	85	19	34	49	63
30	11	24	36	50	86	18	35	52	69
31	18	33	45	56	87	17	34	51	68
32	23	38	51	68	88	18	35	52	69
33	13	24	35	46	89	29	44	58	72
34	26	39	52	65	90	19	37	57	77
35	12	25	40	50	91	22	39	56	73
36	26	40	51	63	92	15	31	46	66
37	11	25	40	53	93	25	43	61	79
38	23	38	51	64	94	20	37	54	71
39	13	30	46	63	95	24	44	65	86
40	21	33	45	56	96	16	32	48	67
41	16	30	46	60	97	18	37	56	77
42	20	37	54	71	98	28	45	63	84
43	16	30	41	52	99	20	38	56	75
44	12	26	35	45	100	27	45	63	82
45	21	37	51	68	101	16	32	52	74
46	26	40	55	70	102	18	34	50	68
47	13	25	37	49	103	16	28	42	56
48	24	38	51	64	104	22	39	56	73
49	23	38	52	67	105	18	34	50	65
50	17	30	44	56	106	20	32	44	56
51	25	39	53	68	107	19	34	49	64
52	19	35	50	64	108	23	39	55	71
53	13	26	39	50	109	15	28	41	53
54	19	35	52	64	110	27	46	65	84
55	14	27	41	54	111	23	39	55	61
56	19	35	51	63	112	20	37	54	71
					113	17	32	47	62

TABLE 2

Successive trials in determining relationship between excursion of recording beam of light (in mm. on photographic paper) from mirror-recorder, and amount of rotation of eyeball in refixating points at 5, 10, 15, and 20 degrees of visual field. Each figure represents average of 15-20 refixations. Subject B.

Date	5°	10°	15°	20°
4/2/31	25	43	61	79
4/3/31	19	40	63	81
4/4/31	22	42	62	82
4/5/31	20	41	62	80
4/6/31	24	44	63	84
Av.	22	42	62	81

varies considerably between some individuals. This individual variability perhaps can be accounted for on the basis of differences in curvature of the cornea upon which the actuation of the mirror-recorder depends.

The figures in Table 2 represent averages of 15-20 refixations at distances of 5, 10, 15, and 20 degrees of the visual field for five successive days for Subject B. Some variability is evident from trial to trial, more so, however, for the 5-degree saccadic movements than for the 10-, 15-, and 20-degree movements. These data indicate the practical usefulness of the mirror-recorder after a certain amount of skill has been obtained in resetting the recorder from trial to trial.

SUMMARY

This is the first report of a series of experimental studies in ocular behavior, and has to do with the adaptation and refinement of methods, technique, and apparatus for an intensive and extensive study of eye-movements.

The validity and feasibility of the Dodge mirror-recorder as to its practical usefulness in photographically recording fine differences in eye-movements on large numbers of subjects was determined on voluntarily initiated eye-movements. The right eye served as the observing eye, while the conjugate movements of the closed left eye were photographically recorded by the mirror-recorder.

The excursions of the beam of light reflected from the mirror-recorder as measured on the photographic record in mm., when plotted against the angular displacement of the eyeball in voluntary refixation of definitely spaced points in the visual field, showed a positive rectilinear relationship. That is, the greater the angular displacement of the eyeball, the greater the excursion of the beam of light on the sensitized paper. This type of relationship was found for all the 113 subjects, although individual variations were evident in the slope of the line of relationship. Some variability was evident in successive refixations for each subject, with the values for the greater portion of the refixations grouped around a central tendency. Likewise, some variability was manifested in successive trials in resetting the mirror-recorder to the closed eyelid for each subject.

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ÉTUDES EXPÉRIMENTALES DU COMPORTEMENT OCULAIRE: I. LE "MIRROR-RECORDER" DE DODGE POUR PHOTOGRAPHIER LES MOUVEMENTS DES YEUX

(Résumé)

C'est l'introduction d'une série d'études expérimentales du comportement oculaire, et décrit l'adaptation et l'amélioration des méthodes, de la technique et des appareils pour une étude intensive et extensive des mouvements des yeux. La validité et la practicabilité du "mirror-recorder" de Dodge (un appareil d'enregistrement photographique) à l'égard de son utilité pratique dans l'enregistrement photographique des petites différences dan les mouvements des yeux chez grand nombre de sujets ont été déterminées pour les mouvements des yeux volontairement initiés. L'oeil droit a servi de l'oeil observant, pendant que les mouvements conjugués de l'oeil gauche fermé ont été enregistrés photographiquement. Les excursions du rayon de lumière reflété du "mirror-recorder" mesurées sur l'enregistrement photographique en mm., quand on les ont mesurées contre le déplacement angulaire du globe de l'oeil dans la refixation volontaire de points espacés d'une manière définite dans le champ visuel, ont montré une relation rectiligne positive. C'est-à-dire, plus le déplacement angulaire du globe de l'oeil est grand, plus l'excursion du rayon sur le papier sensible est grande. On a trouvé ce type de relation pour tous les 113 sujets, quoique des variations individuelles aient été évidentes dans la direction de la ligne de relation. Quelque variabilité a été évidente dans les refixations successifs de chaque sujet, les valeurs de la plus grande partie des refixations étant groupées autour d'une tendance centrale. De même, quelque variabilité s'est manifestée dans les épreuves successives dans le réadjustment du "mirror-recorder" à la paupière fermée de chaque sujet.

Travis

EXPERIMENTELLE UNTERSUCHUNGEN DER AUGENTÄTIGKEIT (OCULAR BEHAVIOR): I. DAS SPIEGELREGISTRIERAPPARAT VON DODGE (DODGE MIRROR-RECORDER)

(Referat)

Wir haben hier eine Einleitung in eine Serie von experimentellen Untersuchungen der Augentätigkeit, worin die Anpassung und Raffinierung der Methoden, der Verfahren, und der Apparate zu einer intensiven und extensiven Untersuchung der Augenbewegungen beschrieben werden. Die Triftigkeit und die Ausführbarkeit des Spiegelregistrierverfahrens von Dodge in Bezug auf seine praktische Nützliehkeit bei der photographischen Regis-

trierung feiner Unterschiede bei den Augenbewegungen einer grossen Zahl von Versuchspersonen wurde an absichtlich ausgelösten Augenbewegungen bestimmt. Das rechte Auge diente als Beobachtungsauge (observing eye) während die verbundenen (conjugate) Bewegungen des geschlossenen linken Auges photographisch registriert wurden. Die Abschweifungen des Lichtstrahls, die von dem Spiegelregistrierapparat widerspiegelt und an der photographischen Aufzeichnung in Millimetern gemessen wurden, erwiesen, wenn sie angezeichnet wurden gegen die Winkelversetzung (angular displacement) des Augapfels bei der absichtlichen Wiederfixierung genau eingeteilter Punkte des Gesichtsfeldes eine positiv geradlinige Beziehung. Das heist, je grösser die Winkelversetzung des Augapfels, desto grösser war die Abschweifung des Strahles auf dem präparierten (sensitized) Papier. Diese Beziehungsart zeigte sich bei allen der 113 Versuchspersonen obwohl in der Neigung (slope) der Beziehungslinie (line of relationship) individuelle Verschiedenheiten zum Schein traten. Es zeigte sich in gewissem Grade Variabilität bei den Wiederfixierungen jeder Versuchsperson. Die Grössen der Mehrzahl der Wiederfixierungen gruppierten sich um eine Zentraltendenz (central tendency). Es zeigte sich ebenfalls etwas Variabilität bei den aufeinanderfolgenden Versuchen im Lauf der Wiederanpassung des Spiegelregistrierapparats an das geschlossene Augenlid bei jeder Versuchsperson.

TRAVIS

AN INVESTIGATION OF CERTAIN FACTORS RELATED TO EASE OF CONDITIONING*

From the Psychological Laboratories of Brown University

Harold Schlosberg

If the conditioned response is to be considered a basic form of learning, as has been assumed by a great many psychologists, there are many questions to be answered. Perhaps one of the more important ones is why it is so difficult to set up stable conditioned responses in adult human subjects. Word associations may sometimes be formed in a single trial, but in conditioning experiments a hundred or more simultaneous presentations of two stimuli are often required before one of them will call out the response originally given to the other stimulus. This may be seen in attempts to condition the lid reflex (3, 5, 8), iris reflex (2), flexion or withdrawal from shock (7, 10), and notably the knee-jerk (5, 17). In a previous paper (14) the writer has described the results of a series of experiments with this reflex. One of the most surprising results of this study was the difficulty of conditioning the knee-jerk, and the great variability from subject to subject in ease of conditioning. Several possible explanations offered themselves, and the purpose of the present study was to investigate some of them.

The first suggestion came from casual observations. On several occasions subjects that had previously shown little evidence of conditioning suddenly gave a large number of conditional kicks when they became disturbed emotionally. One subject, for example, gave no conditioned kicks during his first sitting. He forgot to keep his appointment for the second sitting, and came for it a week late, in a very embarrassed condition. Throughout this sitting he gave many strong conditioned responses. The next day, when he appeared for his third sitting, he was calm again, and gave no traces of conditioning. Other cases of a similar nature might be mentioned. Following this lead, the first part of the present investigation was planned to measure the effect of an experimentally induced period of excitement on the rate of formation of the conditioned knee-jerk.

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*Recommended by Leonard Carmichael and accepted by Carl Murchison of the Editorial Board.

Preliminary Experiments

In general, the fundamental apparatus and methods employed in the preliminary series were similar to those described in a previous paper (14). The subject was seated in a raised chair, with the right leg supported just behind the knee, so the lower leg was free to swing. The conditioning stimulus was furnished by a small single-stroke bell, operated by a foot-key, to leave the experimenter's hands free. The clapper of the bell was adjusted loosely, so that the bell was free to reverberate until the circuit was broken, whereupon it was deadened by a damper on the clapper. The conditioned stimulus was furnished by a gravity pendulum raised by hand, and held up by an electromagnet. The beginning of the clapper stroke broke the circuit through the magnet coils, and permitted the hammer to fall against the patellar tendon. An electric counter was used to keep track of the number of stimulations. The course of events during a double presentation was as follows. The beginning of the conditioning stimulus consisted of a fused click and clang. The bell continued to reverberate. One-third of a second later the hammer hit the tendon of the subject, and the kick followed. After the kick was well under way, the reverberation of the bell was terminated by another click. The height of the kick was not recorded directly during this experiment, but was estimated roughly and noted on a prepared blank.

Ten series of 20 double stimulations were given on each of four successive days. To control "attention" more effectively, as well as to increase the knee-jerk, the subjects were asked to clench the fists around a pair of fist keys, say "Ah," etc., on certain double stimulations. They were given a list of these tasks, and asked to perform the appropriate one each time the bell sounded. A screen hid the experimenter and hammer from the subject. The chief departure from the previous technique was the attempt to disturb the subject with electric shocks. Eight strong shocks were given through the fist keys during one sitting, concurrently with the bell. It was planned to use the shock only during the first sitting with half of the subjects, and during the third sitting with the other half, so that each group might serve as a control for the other. After six subjects had been used, it was seen that the results would not be clear cut. The intensity of the shock apparently could not be regulated to disturb all subjects equally. Some subjects seemed to be little disturbed by a shock strong enough to cause severe wincing, while others reddenned and became garrulous after a very weak shock. Since those who became excited by the shock were the ones who had already been conditioning well, it was feared that no conclusions could be drawn as to the effects of the shock on conditioning. This finding, while discouraging further attempts, at least for the present, along this line, strengthened the belief that some fundamental personality factor was important in determining the ease of conditioning. Previous work, as well as the finding of others, suggested that this characteristic was somehow related to "quickness" or "nervousness," vague as such terms may be. It was decided, therefore, to attempt to find some general measure that would correlate with ease of conditioning.

Three of the subjects described above had not had shocks until the third sitting. Two "normal" sittings were available, therefore, as measures of ease of conditioning. Five more subjects were given two similar sittings. All eight subjects were then ranked in order of ease of conditioning (the term "ease of conditioning" is used throughout instead of "degree of conditioning," since the former implies the use of a limited training period). There were a number of factors on which this ranking could have been made. The height of each conditioned kick had been noted in terms of an arbitrary scale of five points. In addition, early kicks (those starting before the blow) had been noted. It has often been observed that there is a tendency for the conditioned response to come forward in time, toward the conditioned stimulus, so that it actually occurs before the unconditioned stimulus. In certain situations the presence of such early responses has been taken as a sign of conditioning (8, 14, 17). In the present situation the absence of such early kicks should not be taken to prove that no conditioning has taken place. It is always possible that the subject is well conditioned, but that this displacement in time has not occurred. In this case, the only measure of conditioning available is the occurrence and height of conditioned kicks on trials when the blow has been withheld. Therefore, the subjects were ranked on the total height of the conditioned knee-jerks that each one exhibited in 16 trials, scattered throughout the two sittings. This method is not ideal, because of the small number of trials, but a larger number could not have been given without danger of subjecting the conditioned response to "experimental extinction."

Each of these subjects was then given the following tests.[1] The results were correlated against ease of conditioning.

1. Reaction-times. This type of measure was chosen in an attempt to get at the element referred to above as "quickness." No theoretical relationships between reaction-time, speed of the nerve impulse, and intelligence, were implied. These measures were taken in an attempt to determine certain general characteristics of the individuals' response to simple stimuli. For this reason the instructions were general, permitting the subject to take the "motor" or "sensory" attitude, as he naturally acted. The actual measures taken were as follows:

a. Simple auditory reaction, with the Dunlap reaction board. Forty trials were taken with each subject.

b. Choice reaction, with the Dunlap apparatus. The subjects were requested to squeeze the bulb when the green light appeared, but not when the white light was shown. Errors were counted when the bulb was squeezed in response to the white light. Twenty reactions to the green light, interspersed with 20 presentations of the white, were measured.

c. Since all reactions with the Dunlap board were unusually short, it was feared that the clutch was slipping. The auditory reactions were repeated on a synchronous motor chronograph.[2] It was found that the reaction-times, as measured by the Dunlap, were too short, but that the rank of the subjects was little disturbed.

2. Timing Test. This group of tests was similar to one mentioned by Miles (11). A stop-watch, reading in hundredths of a second, was handed to the subject. The large-sweep hand made one revolution in three seconds. The dial was calibrated in hundredths of a second, and the one- and two-second points were plainly indicated. The subject was shown how to operate the watch, and asked

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[1] The results of one subject (not the same one each time) were not available on each test. Hence the number reduced to seven for each test.

[2] This instrument was developed and described by Professor Delabarre thirty years ago (4). It consists essentially of a smoked drum, rotated by a synchronous motor. For auditory reactions, the experimenter's movement permits a stylus to drop onto the drum. The click of the impact acts as the stimulus, and the response mechanically lifts the recording point. The resulting line can then be measured, with a tape, and the reaction-time read to 1/100 of a second. I am greatly indebted to Professor Delabarre, and to E. B. Delabarre, Jr., for setting up the apparatus (it had been dismantled for several years, because alternating current was not available) and for taking these reaction-times.

to start it, and then stop it with the hand pointing exactly at the two-second mark. The actual reading was then noted. Twenty such trials were made by each subject. This test was included in an attempt to measure the ability of the subject to respond accurately at the end of a time interval. It was felt that this might be important, since there was an interval of 1/3 second between bell and blow. Correlations were calculated with the average reading, and with the average error, measured either side of two seconds.

3. The Downey Will-Temperament Test (6). Each subject was given the individual form of the test, and correlations made with each item. The test was included because it involved a number of motor tasks executed under various conditions and furnished norms for their performance.

Correlations were calculated between ease of conditioning and a large number of different aspects of the tests given. The correlations obtained are not listed. Because of the small number of subjects, coupled with the nature of certain of the tests, no great significance can be placed on any one correlation. The simple reaction-time, and certain items of the Downey test, seemed most promising. It was decided, therefore, to repeat them with a larger number of subjects, and to add certain items to the battery of tests given to the main group.

Main Series

Twenty subjects, all men, were used in this experiment. Five of them were graduate students, the rest undergraduates. All of them were members of a course in physiological psychology, and had had other courses in psychology. They were given one sitting, of 200 double stimulations, with almost the same procedure as that used during the first sitting of the previous group. The only change in the procedure, from the subject's viewpoint, was the addition of 20 stimulations before, and 20 after the regular sitting. In each of these additional series, bell and blow were given together 10 times and the blow alone the other 10. No "voluntary" response was called for during these two series. They were added to the usual sitting to furnish a measure of the unconditioned reflex, and the facilitating effect of the bell. The apparatus was slightly modified, to permit measurement of the height of the kick. A light harness was pivoted on the leg rest, and attached to the leg of the subject. A pointer attached to this harness swept over a quadrant calibrated in degrees, and arranged so that it was visible to the experimenter, as he sat in front of the subject. The scale was movable, so that the zero point could be adjusted for each subject. After a little practice, the experimenter was able to read and note down the height of each kick, and the appearance of early kicks. These were noted on a mimeographed blank, as in the preliminary experiment. This method is more convenient than direct recording by movement or muscle thickening, and is satisfactory for the present purposes.

After the conditioning sitting was finished, each subject was given 50 simple auditory reactions. For this purpose a Cenco impulse counter was used (11). It was operated from the regulated phase 60-cycle A.C. lighting circuit, stepped down to 8 volts by a bell-ringing transformer. The subject was requested to release his key when he heard the buzz of the counter. A screen hid the experimenter's hand, and the key was depressed slowly and gradually, so that no warning, other than the word "ready," was given to the subject. This impulse counter, when operated by 60-cycle current, reads in 1/120 second, and is extremely convenient and rapid to use. Because of its construction, it may lose a unit at the beginning or end of the interval measured, depending upon how it hits the phase of the current. This makes it inadvisable to use the instrument as a measure of variability, but does not seriously affect its value where the readings are to be averaged and used in computing relative reaction-times. In Table 1, the correlation between ease of conditioning and speed of reaction-time is given.

An attempt to measure "suggestibility" was made immediately after the reaction series. The sway technique described by Hull (9) was used. The subject was asked to stand, with heels together, and back turned toward a kymograph. Sway was recorded by attaching a thread to his coat collar, reducing its movement to 1/4 by a pair of pulleys, and recording it on a slowly moving smoked drum. No attempt was made to keep the subject from knowing that his sway was being recorded. Records were first taken for two 1/2-minute periods without suggestion, eyes open during one, and closed during the other. Two 2-minute records were then taken, with eyes closed, one with the suggestion of forward sway, and the other, backward. The results from these records will be described later.

Several weeks after the above tests, the subjects were given the Bernreuter Personality Inventory (1) during a class hour. This inventory was chosen because it permitted several personality meas-

TABLE 1

Correlations between Ease of Conditioning and Other Measurements

Measurements correlated with ease of conditioning	r	$P.E._r$
Average height of unfacilitated knee-jerk in preliminary series	.01	.16
Average height of knee-jerk, Series VI	.37	.13
Facilitating effect of bell before conditioning	.60	.10
Facilitating effect of bell after conditioning	.45	.13
Change in facilitating effect of bell caused by conditioning	.32	.14
Bernreuter Inventory:		
B1-N	.03	.16
B2-S	.03	.16
B4-D	.09	.16
Average reaction-time, 50 trials	.38	.13
Downey Will-Temperament Test:		
Speed of Movement	.41	.14
Freedom from Load	.39	.14
Flexibility	.24	.16
Speed of Decision	.04	.17
Motor Impulsion	.38	.14
Reaction to Contradiction	.09	.16
Resistance to Opposition	.19	.16
Finality of Judgment	.06	.16
Motor Inhibition	.09	.16
Interest in Detail	—.23	.16
Coordination of Impulses	—.04	.17
Volitional Perseveration	—.13	.16

urements to be made from the same blank. It was thought that one of these might correlate with ease of conditioning. The correlations obtained are given in Table 1.

About a month later, 18 of the original 20 subjects were given the Downey test (individually). The other two subjects were not available. Correlations with each of the items are given in Table 1.

Results and Discussion

As has been noted during the description of the tests, correlations were made between ease of conditioning, and certain results obtained from the tests (see Table 1). The basis for ranking the subjects in ease of conditioning has not been given. As we have pointed out above, two fundamental measures of conditioning were available, early kicks and conditioned kicks. These two measures show a correlation of .66 ($P.E. = .09$) with each other. It would probably be much higher if more trials for the conditioned response could have been made. It will be noted that only one sitting was used with this group of subjects, so that fewer trials of the conditioned response were available than in the preliminary experiments. To make up for this lack, a composite ranking was made in which both early kicks and conditioned kicks were considered. This composite rank is undoubtedly a better measure of ease of conditioning than either of the single ranks. It is this composite rank that was correlated with the results of the other measures. Correlations were computed by the rank difference method and were converted into *r*.

The first significant correlation one might expect to find would be between strength of the unconditioned reflex and ease of conditioning. Pavlov has found this to be true with clocklike regularity (12). But if we examine the correlation between the first ten unfacilitated kicks of the preliminary series (before conditioning had taken place) and ease of conditioning, we find that the correlation is practically zero. If we rank the subjects on the basis of the average height of the kicks in a series in the middle of the sitting, it is probable that we will get a more reliable measure of the relative height of their knee-jerks, for by this time they will have become better adjusted to the situation. The correlation between ease of conditioning and height of knee-jerks during the sixth series is .37, about three times its *P.E.* How much of this correlation is due to the facilitating effects of the bell and voluntary response cannot be said. The data mentioned in the next paragraph suggest that the facilitating effects of the bell account for most of the correlation.

The highest correlation obtained, except for that between the two measures of conditioning, is that between ease of conditioning and the amount of facilitation or inhibition exercised by the bell in the preliminary series. In this series, the average of the ten responses to the bell-blow stimulation was divided by the average of the ten responses to the blow alone, and the resulting figure called the "percentage of facilitation." This varied between 53% and 290%. Values above 100% mean that the bell facilitated the response to the blow, while those below 100% show that it had an inhibiting effect, with the particular interval (1/3 second) used between bell and blow. The correlation between ease of conditioning and original facilitating effect of the bell was .60. Thus it may be said that, to this extent, the greater the original facilitating effect of the new stimulus, the greater and easier will be the conditioning. These results are quite in accordance with those of Wendt (17), who found that conditioning was easier if the new stimulus had a strong facilitating effect on the response. The conditions in his experiment were somewhat different from those of the present one, and the conditioning to which he refers may not be strictly comparable to that with which we are dealing. It seems doubtful, however, that the process of conditioning is fundamentally different in the two cases.

Without accepting any particular physiological theory of facilitation and inhibition, one might say that, before the formal conditioning begins, there is a functional relationship between the bell stimulus and the kick. This relationship, or connection, in the situation with which we are dealing, is not enough to actually elicit an overt movement of the leg, but may be strong enough to increase, or, in other cases, to decrease the response to some other stimulus, as the blow. This effect varies from subject to subject, and with different intervals between bell and blow. Repetition of this sequence of events a number of times might be expected to increase the effects of the bell stimulus on the leg muscles, so that, in certain cases, the effect of the bell may be enough to cause overt movement of the leg. It is this movement, then, that would be called a "conditioned response."

That this does not include all the factors, however, is clear from other correlations. After the conditioning period, the correlation between ease of conditioning and bell facilitation has fallen to .45. The low value of this figure is surprising, for one might have expected the correlation to be as high as that between the two basic measures of conditioning. Indeed, one might expect to find that the increase in facilitating effect of the conditioned stimulus was the best measure of extent of conditioning. But when the percentage of facilitation is calculated for the trials before the main conditioning series, and subtracted from that obtained after the series, the difference obtained correlates rather poorly (.32) with ease of conditioning. Therefore, one can readily see that the conditioning does not consist simply of increasing the facilitating or inhibiting effect of the new stimulus. The correlations obtained suggest that the original effect of the extra stimulus on the unconditioned reflex is very important in determining the ease of conditioning, but that other factors are also of great importance.

The attempt to find any of these other factors has not been completely successful. One of the most likely factors seemed to be that complex of processes known as suggestibility. The conditioned knee-jerk, in many ways, resembles an automatism. The shape of the curve of contraction and the latent time resemble closely those obtained from "voluntary" kicks, where the subject was asked to kick as soon as he heard a sound. But in the conditioning experiments, great care was always taken to give the subject no verbal instructions, either to kick, or not to kick. If the subject asked whether or not he was supposed to kick, he was always told that it didn't matter, and asked to pay no attention to his leg, but to concentrate on responding to the bell, according to the list furnished to him. But such instructions, in many cases, merely evade the issue, and force the subject to set his own instructions. It is very doubtful that any subject consciously "set himself" to kick in response to the bell. Although most subjects denied emphatically that the kicks were "voluntary," one or two have felt that the conditioned kick, especially after a long and successful conditioning, was semi-voluntary. Thus one subject who (in spite of the usual conditioned response theory) had given uniformly brisk conditioned reflexes for 50 trials without reinforcement remarked: "I feel as if I could stop the kick, if I wanted to, but I am just letting it go on." He was able to inhibit it when requested to do so. However, another subject who was giving similar results was unable to inhibit the kick, when he tried to.[3] Thus the conditioned knee-jerk seems to be a bit of behavior more or less broken off from our usually integrated patterns. In a way, this is what we would expect when we encourage a subject to pay no attention to the conditioning process, or at least ask him not to kick "voluntarily." Indeed, we even set him to doing something else, as a distraction. This is markedly different from the methods used with young children and lower animals, where every effort is made to avoid distracting stimuli, and to focus the organism on the stimuli and responses involved in the conditioning. But if this method were adopted with adult human subjects, we should be forced to classify the experiment under some heading other than that of "conditioned response." In other words, when we are working with the skeletal-muscle responses of human beings, most of our efforts are devoted to breaking off the stimulus-response sequence from the rest of their behavior, lest the conditioned response formed be called "voluntary." This may explain why it is so difficult to set up a conditioned response such as withdrawal from shock, knee-jerk, wink, etc., in adult human subjects. Furthermore, it would suggest that the individuals who condition most readily are those who are able to form fractional bits of behavior.

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[3] Wendt (17) reports results similar to those mentioned in this paragraph. The papers by Helen Peak (13) and Hilgard (8) are also pertinent.

It seemed possible that the sway test for suggestibility would measure this tendency, and therefore correlate well with ease of conditioning. The results, however, were disappointing. The only subject to lose his balance as a result of the suggestions comes in the upper five in ease of conditioning. But no consistent results were obtained. Whether this was due to the inherent limitations of the method, or to lack of skill on the part of the experimenter in giving the suggestions, cannot be stated. With the exception of the one subject mentioned above, no outstanding differences were noted between the records obtained.

The correlations made with the Bernreuter Personality Inventory were equally unsatisfactory. All three calculated were near zero, and less than their *P.E.* Apparently this inventory does not measure a factor that correlates with ease of conditioning. There is, of course, the possibility that such an inventory is not valid when administered to individuals of some sophistication in psychology.

Only three items on the Downey test yielded correlations greater than twice their *P.E.* One of them, Motor Impulsion, might be expected to be related to ease of conditioning, but perhaps the size of the coefficient ($r = .38$), and the number of cases ($n = 18$), make it inadvisable to discuss the matter further. Two of the other items gave correlations of almost three times their *P.E.*, namely, Speed of Movement ($r = .41$), and Freedom from Load ($r = .39$). These items are entirely dependent upon speed of writing. It will also be noted that the average auditory reaction-time yields about the same results ($r = .38$) when correlated with ease of conditioning.

Because only 20 subjects were used, no one of these correlations was more than three times its *P.E.* However, the same relationship seemed to hold in the preliminary experiments. It seems likely, then, that the correlation is a valid one, and that quicker individuals condition more readily, at least under the conditions of the present experiment. It is possible, of course, that this relationship is dependent upon the particular interval used between bell and blow. But it is interesting to note that several years ago (14), the writer ventured the suggestion that the subjects who conditioned most readily were "of the quick, 'nervous' type." Switzer (15), too, working largely with the lid reflex, uses the phrase "quick, 'nervous' and suggestible types" to apply to those who condition well. Whether or not speed of movement and reaction is related to this "quickness," and whether it, in turn, is related to a tendency to form isolated bits of behavior, split off from our normal integrated activity, cannot be answered with assurance. And just what relationships these findings bear to Pavlov's "excitable" dogs (12) remains to be investigated. A study of ease of conditioning in certain types of mentally deranged patients is one possible line of experimentation. Undoubtedly, further work must be done in the direction of clarifying the meaning of "quick," "excitable," "suggestible," and, above all, "nervous." The answer to these questions is of fundamental importance if we are to understand the relationship between conditioning and other forms of learning in the adult human being. Many psychologists now hold that the conditioned response is not a fundamental type of learning and would agree with Thorndike's hypothesis that "The phenomena of the pure conditioned response will teach us more about excitability than about learning" (16, p. 112). The available evidence neither affirms nor disproves this view conclusively. It is possible, however, that Thorndike's remark holds true only in those cases where conditioning takes place in a complicated organism where the conditioning situation favors the formation of an isolated pattern. Regardless of whether or not this happens with Pavlov's dogs, the writer is firmly convinced that it does happen in most experimental attempts to condition adult human subjects. Most of the muscles we use in conditioning experiments have been used since birth in conjunction with our general integrated activity. For example, we kick at objects, and extend the leg in walking and on command. In our attempt to get away from "voluntary" action, and to get objective examples of conditioning, we deliberately disregard this mass of coordinated behavior patterns and attempt to set up an isolated relationship between a new stimulus and this response, often directly opposed to the patterns already formed. The writer does not suggest that these findings account for all the difficulties encountered in applying Pavlov's results to adult human learning. It seems likely, however, that the isolation of the conditioned response is an extremely important factor, which helps us to understand the slowness and uncertainty we find when we attempt to set up conditioned responses involving skeletal musculature in adult human subjects.

Conclusions

1. The ease with which the knee-jerk may be conditioned to a bell varies greatly from subject to subject.

2. There is a fairly marked correlation between ease of conditioning and the original facilitating effect of the bell. If the facilitating effect is measured after the conditioning period, the correlation is smaller.

3. There is a positive correlation between speed of reaction and ease of conditioning.

4. The results described in this paper support the view that the conditioned response established in this and other similar situations is split off from the rest of the subject's activity, like an automatism, and that this splitting-off helps to explain the difficulty of establishing such conditioned reflexes.

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UNE ÉTUDE DE CERTAINS FACTEURS QUI ONT RAPPORT À LA FACILITÉ DU CONDITIONNEMENT

(Résumé)

Dans l'expérience principale, vingt sujets ont subi 200 stimulations doubles, consistant du son d'un timbre, suivi dans le tiers d'une seconde d'un coup sur le tendon patellaire. Puis on a rangé les sujets selon la facilité de leur conditionnement. On a fait des corrélations entre cet ordre de rang, et ceux obtenus de certaines mesures. La facilité du conditionnement a donné une bonne corrélation avec le premier effet facilitant du timbre sur le réflexe rotulien. Il se trouve aussi une corrélation entre facilité du conditionnement et certaines mesures de vitesse, comme le simple temps de réaction, et les divisions I et II du Test de Volonté-Tempérament de Downey. Certaines autres mesures ont donné des coefficients de peu de valeur ou de nulle valeur.

Il semble probable que l'effort d'éliminer les éléments "volontaires" dans la formation d'une réponse conditionnelle chez un être humain adulte oppose les formes nouvelles de comportement à celles formées préalablement dans la vie. On s'attendrait donc à trouver le conditionnement difficile dans telles conditions. D'ailleurs il serait probable que toutes les réponses conditionnelles ainsi formées seraient "fendues" du reste du comportement du sujet, et ressembleraient à un automatisme. On pourrait croire que les sujets "prompts à agir," "émotifs," forment de tels automatismes le plus facilement. Les résultats de cette expérience témoignent que ces sujets forment le plus souvent des réflexes rotuliens conditionnels lesquels sont stables.

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EINE UNTERSUCHUNG GEWISSER EINWIRKUNGEN, DIE ZUR LEICHTIGKEIT DER BEDINGUNG (EASE OF CONDITIONING) IN BEZIEHUNG STEHEN

(Referat)

Im Hauptversuch wurden 20 Versuchspersonen mit je 200 Doppelreizungen gereizt, die aus einem Glockenschall bestanden, der nach einer 1/3 Sekunde von einem Schlag auf die Kniesehne (patellar tendon) gefolgt wurde. Die VPP. wurden dann in Bezug auf die Leichtigkeit ihrer Bedingung rangmässig geordnet. Es wurden Korrelationen berechnet, zwischen dieser Rangordnung und Rangordnungen die an gewissen anderen Massstaben berechnet worden waren. Die Leichtigkeit der Bedingung

erwies eine hohe Korrelation mit der Stärke der ursprunglichen fördernden Einwirkung der Glocke auf die Kniesehnerzuckung (knee-jerk). Es besteht ebenfalls eine Korrelation zwischen der Leichtigkeit der Bedingung und gewissen Massstaben der Schnelligkeit, wie z. B. die einfache Reaktionzeit (simple reaction-time) und die ersten zwei Gegenstände des Downey Will-Temperament Test. Gewisse andere Messungen ergaben Koeffizienten, die wenig oder keinen Wert besassen.

Es ist wahrscheinlich, dass der Versuch, willkürliche Einwirkungen bei der Bildung einer bedingten Reaktion bei einem erwachsenen Menschen auszuschalten, die neu-gebildeten Reaktionsgestalten (behavior patterns) den früher im Leben gebildeten entgegenstellt. Man würde deshalb erwarten, dass die Bedingung unter diesen Umständen schwierig sein würde. Ferner würde es wahrscheinlich sein, dass auf diese Weise gebildete bedingte Reaktionen vom übrigen Benehmen der Vp abgesondert sein würden, und einem Automatismus gleichen würden. Es ist zu erwarten, dass rasch reagierende, erregbare VPP solche Automatismen am leichtesten bilden würden. Die Ergebnisse dieser Untersuchung weisen darauf hin, dass solche VPP eher als andere VPP beharrliche (stable) bedingte Kniesehnenzuckungen (conditioned knee-jerks) bilden.

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RETROACTIVE INHIBITION AND HYPNOSIS*[1]

From the Psychological Laboratories of Yale University

MILDRED B. MITCHELL

PART I. ALLEGED DISSOCIATION OF MEMORIES IN HYPNOSIS

Introduction. The dissociation theory of hypnosis as expounded by such writers as Pierre Janet (2) would lead one to believe that material learned in a waking state might not have any retroactive effect on material learned in a waking state, and vice versa. The retroactive effect of similar materials learned in the waking state has been well established by Robinson and others. It was, therefore, the desire of the present writer to ascertain whether or not these retroactive effects still occurred when the interpolated material was learned in a different state from the original learning. It was assumed that if a complete dissociation of memories occurred when the subject was put into a trance—especially when put into a very deep trance—these effects would disappear. In other words, there would be more saving in relearning when there was a change from one state to another between the memorization of the original and interpolated material than when the memorization was all done in the same state, either waking or trance.

Material, Method, and Apparatus. The material for memorization in this experiment was lists of ten three-place numbers.[2] The *anticipation* method was used by presenting the material on Hull's memory drum. A 2″ exposure was used for the presentation of each number. Since there were 12 exposures to one revolution of this drum, and 10 numbers in the list, a 4″ interval elapsed between trials.

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[1] This experiment was performed in the psychological laboratory of Yale University under the direction of Professor Edward S. Robinson and with the advice and assistance of Professor Clark L. Hull. The writer is indebted to Professor Hull for use of his apparatus. Part I of this article was reported at the American Psychological Association meeting in Toronto, 1931.

[2] A more detailed explanation of this method and material is given in the writer's doctorate dissertation (at Yale, 1931): *Errors in the Memorization of Numbers,* on deposit in the Yale University Library.

A 2′ interval was allowed between the learning of the lists to give ample time to get the subjects from one state, either waking or trance, into the other. Each list was learned to one perfect repetition.

Subjects. The subjects used were two men. One, Fus, was a junior in Yale College, and the other, Beh, a graduate student in psychology at Yale. The subjects were both volunteers and were not paid for their work. Subject Beh went very readily into a deep trance after which he showed complete amnesia as tested by Strickler's (8) method, and practically no spontaneous movement or initiative. For instance, when he had completed his task, the memorization to one perfect repetition, he made no sign of recognizing this fact, but seemed automatically to continue until the apparatus was stopped. Fus, on the other hand, while going into a trance quickly, showed varying degrees of amnesia, and more spontaneous movement. Part of the difference may have been due to the fact that Fus was near-sighted and had to bring his eyes close to the apparatus to see the numbers, while Beh could remain with his head resting comfortably against the back of the morris chair, a position which he habitually assumed when going into a trance.

Procedure. The general procedure was that of a retroactive inhibition set-up. During the daily sessions, each subject learned two lists of numbers and relearned the first list. When the learning was done in a trance, the subject was always given a suggestion of post-hypnotic amnesia for everything that had happened during the trance before being brought back into a waking state.

The work reported here may be divided into two parts. The first, I shall call Series I for convenience. Condition A of Series I contained a change from the waking state to the trance after the learning of one list of numbers and another change back to the waking state for the relearning. The control on this, Condition B, consisted of learning both lists and of relearning the first list in the waking state. It may be abbreviated thus:

SERIES I

Condition	O.L. List 1	Interpolation (List 2)	Rel. List 1
A	Waking	Trance	Waking
B	Waking	Waking	Waking

Series II was the opposite of Series I. Condition A contained a change from the trance to the waking state after the learning of one list of numbers and another change back to the trance state for the relearning. The control on this, Condition B, consisted of learning both lists and of relearning the first list in the trance state. It may be abbreviated thus:

SERIES II

Condition	O.L. List 1	Interpolation (List 2)	Rel. List 1
A	Trance	Waking	Trance
B	Trance	Trance	Trance

This second series was run because, according to Morton Prince (4), one integrate of a personality might have its memories dissociated from another without the converse being true. In other words, even though a list learned in a trance might not interfere with a list learned in a waking state, a list learned in a waking state might interfere with one learned in a trance, and vice versa.

To summarize: All the work in Series I was done in the waking state except the learning of the interpolated list in Condition A. In either series, if there were a dissociation of memories between the two states, then one would expect to find a greater saving in the relearning for Condition A than for Condition B.

Each series consisted of 8 days (after a practice period of 4 days). One condition was presented each day in the order ABBAABBA for one subject, and the reverse of this for the other subject to eliminate practice effects.

Results. The results for Series I are summarized in Table 1. The savings scores are all in terms of percentage of saving figured by the formula:

$$\text{Savings Score} = 100 - \frac{\text{Relearning trials}}{\text{Learning trials}} \times 100.$$

In the case of saving in terms of errors, the word *errors* was substituted in this formula for *trials.* The recall score was obtained by multiplying the number of responses correct on the first trial of the relearning by 10. It was too small (usually 0) to be significant.

A comparison of the mean scores for Conditions A and B in terms of saving in trials for subject Beh shows, not a greater saving for Condition A, as might have been expected if there had been a dissociation between the waking and trance states, but, on the contrary, a greater saving for Condition B. There was a mean saving of 47% for Condition B and only 35% for Condition A. However, an examination of the scores for each of the 4 sessions included in each mean shows that the probable error is very great. Similarly, in terms of savings in errors, there was a larger saving for Condition B when there was no change of state. The mean score was 45 for Condition B and 38 for Condition A.

When the corresponding scores for subject Fus are examined, it can be seen that they agree with those for subject Beh, but are much exaggerated and suggest a more reliable difference between the two conditions.

The results for Series II are summarized in Table 2. The scores are figured in the same manner as are those for Table 1. Here again the recall scores are too low to be significant.

TABLE 1

SERIES I

The numbers are all in terms of percentages. The conditions are:

Condition	O.L. List 1	Interpolation (List 2)	Rel. List 1
A	Waking	Trance	Waking
B	Waking	Waking	Waking

Condition	Savings in trials A	Savings in trials B	Recall score A	Recall score B	Saving in errors A	Saving in errors B
			Subject: *Beh*			
	33	57	0	0	20	42
	36	20	0	0	36	48
	50	67	0	0	39	63
	20	43	30	0	55	28
Σ	139	187	30	0	150	181
Mean	35	47	8	0	38	45
			Subject: *Fus*			
	—13	50	0	20	—2	55
	0	64	10	20	29	65
	—14	0	0	0	—57	—13
	44	80	0	0	46	79
Σ	17	194	10	40	16	186
Mean	4	49	3	10	4	47

TABLE 2

SERIES II

The numbers are all in terms of percentages. The conditions are:

Condition	O.L. List 1	Interpolation (List 2)	Rel. List 2
A	Trance	Waking	Trance
B	Trance	Trance	Trance

Condition	Saving in trials A	Saving in trials B	Recall score A	Recall score B	Saving in errors A	Saving in errors B
			Subject: *Beh*			
	0	0	0	0	24	—4
	44	75	0	20	42	89
	17	29	0	0	20	30
	—20	50	0	0	—3	74
Σ	41	154	0	20	83	189
Mean	10	39	0	5	21	47
			Subject: *Fus*			
	12	29	10	10	—29	16
	65	—17	0	0	64	—17
	20	33	0	0	8	42
	—50	—17	0	0	—72	7
Σ	47	28	10	10	—29	48
Mean	12	7	3	3	—7	12

A comparison of the mean scores for Conditions A and B in terms of saving in trials for subject Beh again fails to show a greater saving for Condition A. The scores show even a greater difference than in Series I. In this case the mean saving in trials for Condition A is 10 and for Condition B is 39. The results are similar in terms of saving in errors.

The corresponding scores for Fus, who during this part of the experiment seemed to be going into a much lighter trance with less amnesia, showed an extremely unreliable difference in the two conditions in terms of saving in trials. The mean score was 12 for Condition A and 7 for Condition B. This was the only mean score showing a greater saving for Condition A. Even here the corresponding scores in terms of savings in errors showed a greater saving for Condition B; 12 for Condition B as contrasted with —7 for Condition A.

Since the greater disturbances in memory occurred when there was a change of state, it was thought that the results might be due to an unobservable emotional upset experienced during the change itself. As a little check on this, at the end of each series, a session was held in which the subject was run through a condition similar to Condition A, i.e., with a change in state, but, instead of learning a second list, the subject was allowed to rest for 6 minutes. In these cases, for each of the subjects, the savings in both errors and trials were not only greater than the mean score for either Condition A or B, but were also greater than the score for any individual session of Condition A. If the experiment were repeated, this control should be run as a regular condition. It is essentially in agreement with the findings of Jenkins and Dallenbach (3). Their subject showed perfect recall in the trance for material learned from one to eight hours previously in the trance although the intervening time was spent in the waking state.

Conclusions. The results, such as they are, are purely negative. No positive evidence was found to uphold the dissociation theory of hypnosis. The differences, if found upon repetition to be reliable, will show that learning the interpolated list of numbers in a changed state, either waking or trance, causes more interference in the recall of the list, than learning them in the same state. Thus they are not completely dissociated, although there is an apparent amnesia. The writer realizes the statistical unreliability and the tentativeness of any conclusions that may be drawn from the use of two subjects for such a brief experimental period as 22 days. For this reason, when circumstances beyond the control of the experimenter made it impossible to continue the investigation, this material was not used for a doctorate dissertation. It was then hoped that the work could be resumed in the near future, but, since that has not proved possible, this is written to set forth the need for further experimentation.

Part II. Alleged Super-Memory in Hypnosis

Introduction. Marvellous feats of memory have often been attributed to the phenomenon of hypnosis. Young (10) found some experimental evidence for the superiority of the trance state over the waking state in recalling remote events such as childhood incidents. He found no differences between the two states in the recall of articles in an adjoining room that had been seen many times. He found memory for nonsense syllables slightly better in his somnambulistic subjects. Huse (1) found no reliable difference between the waking and trance states for recall of nonsense material learned the preceding day. What little difference there was favored the waking state. Stalnaker (7) found some improvement in the trance for poetry memorized years before.

The experiment here reported was set up to test the power of memory in hypnosis for material recently acquired as compared with memory in the waking state for this material.

Material, Method, and Apparatus. The material consisted of lists of ten three-place numbers. The *anticipation* method was used by presenting the material on McGeoch's memory drum. A 2″ exposure was used for the presentation of each number. Since the drum has 20 exposures per revolution each list was typed twice on a large sheet containing 8 different lists. Thus no time was allowed between trials. The last number of the list became the cue for the first number of the list. A 2′ interval was allowed between the learning of the lists to give ample time to get the subjects from one state, either waking or trance, into the other. Each list was learned to one perfect repetition.

Subjects. The subjects were the same two used in the previous work presented in Part I of this article. They were both volunteers and continued to work without pay.

Procedure. The procedure in general was that of a regular retroactive inhibition set-up with controls. During the daily sessions each subject learned one list of numbers in the waking state, then either learned a second list or did some light reading in the waking state, and finally relearned the first list either in the waking or trance state. There were, therefore, four conditions which might be summarized as:

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

In other words, during Condition A, the subject learned a list of numbers in the waking state, read for seven minutes, and then relearned the first list. In Condition B, he learned two lists of numbers and relearned the first list, all in the waking state. In Condition C, he learned a list of numbers, read for five minutes, was put

TABLE 3
SAVING IN TRIALS FOR SUBJECT BEH

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

	Condition			
	A	B	C	D
		Cycle 1		
	95	13	56	—40
	60	67	29	0
	78	56	85	—14
	60	45	0	57
Σ	293	181	170	3
Mean	83	45	43	1
		Cycle 2		
	89	38	20	—29
	67	44	20	—60
	45	33	50	14
	60	14	82	71
Σ	261	129	172	—3
Mean	65	32	43	—1
		Cycle 3		
	—17	29	75	14
	—75	0	67	—100
	90	0	67	22
	67	50	29	70
Σ	65	79	238	6
Mean	16	20	60	2
		Cycle 4		
	40	68	57	20
	80	—68	55	—40
	90	14	80	33
	100	43	60	14
Σ	310	57	252	27
Mean	78	14	63	7
		Cycles 1-4		
Mean	58	28	52	2

into a trance and relearned the first list. Finally, in Condition D, he learned two lists in the waking state, was put into a trance and relearned the first list. Since both subjects had already served as subjects in a similar experiment, only a two-days' practice was necessary to get them accustomed to the new apparatus. One condition was presented each day in the order ABCDDCBABADCCDAB to Beh and in the order DCBABADCCDABABABCD to Fus to eliminate any practice effects which might still be present. This gave a convenient cycle of 16 sessions in which each condition was presented 4 times. The experiment was continued until both subjects had been put through 4 such cycles or 64 sessions, with each condition presented 16 times.

From previous experiments on retroactive inhibition (5, 6), a larger saving would be expected for Condition A than for Condition B and also more for Condition C than for Condition D, although this had not been done using a trance state. The objective here was primarily a comparison of Condition A with C, and Condition B with D. If there were a significant improvement of memory in the trance state for recently learned material, then the subject should show a greater saving for Condition C than for A, and a greater saving for Condition D than for B.

Results. The results are presented in terms of savings in trials, savings in errors, and recall scores, as described in Part I. Tables 3 and 4 give the results in terms of saving in trials for subjects Beh and Fus, respectively. For both subjects, the expected differences between Conditions A and B and between C and D were found. After learning a second list of numbers, more trials were needed to relearn the first list than when a rest period was interpolated between the original learning and the relearning. Beh (Table 3) shows a consistent difference for each of the 4 cycles between Conditions B and D. The saving for Condition D is not only less than for Condition C, but it is also less than for Condition B. This is in agreement with the results previously obtained on this subject. When there was a change from one state to another, there was a decrease in saving score during the relearning. Fus, on the other hand, showed practically the same amount of saving for Conditions B and D. As stated in Part I, Fus was always in a state more nearly identical with the waking state than was Beh. This probably accounts for the differences between the two subjects and the similarity of Fus's results for the two states.

TABLE 4
SAVING IN ERRORS FOR SUBJECT FUS

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

	Condition			
	A	B	C	D
		Cycle 1		
	93	40	71	87
	40	50	50	25
	60	0	50	50
	80	57	80	50
Σ	273	147	251	212
Mean	68	37	63	53
		Cycle 2		
	80	50	14	0
	0	0	25	71
	100	63	100	0
	90	33	83	73
Σ	270	146	222	144
Mean	68	37	56	36
		Cycle 3		
	70	43	100	29
	100	13	90	33
	60	33	90	—40
	100	25	100	50
Σ	330	114	380	72
Mean	83	29	95	18
		Cycle 4		
	100	40	100	—20
	57	29	87	17
	—20	—33	89	60
	100	50	100	38
Σ	237	85	377	95
Mean	59	21	94	24
		Cycles 1-4		
Mean	69	31	77	33

TABLE 5

SAVING IN TRIALS FOR SUBJECT BEH

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

	Condition			
	A	B	C	D
		Cycle 1		
	95	37	29	0
	81	52	6	—24
	83	50	78	21
	80	43	18	36
Σ	339	182	131	33
Mean	85	46	33	8
		Cycle 2		
	87	53	36	—9
	63	18	30	—59
	68	30	38	—25
	81	15	64	61
Σ	299	116	168	—32
Mean	75	29	42	—8
		Cycle 3		
	31	24	72	10
	—41	—31	62	—65
	93	0	58	—9
	85	54	21	62
Σ	168	47	213	—2
Mean	42	12	53	—1
		Cycle 4		
	68	55	49	7
	92	—27	66	—39
	90	17	74	8
	100	54	61	28
Σ	350	99	250	4
Mean	88	25	63	1
		Cycles 1-4		
Mean	72	28	48	0

TABLE 6
Saving in Errors for Subject Fus

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

	Condition			
	A	B	C	D
Cycle 1				
	93	39	68	82
	57	49	63	10
	59	—15	66	56
	81	62	79	52
Σ	290	135	276	200
Mean	73	34	69	50
Cycle 2				
	79	50	52	18
	39	18	52	59
	100	50	100	11
	84	37	93	69
Σ	302	155	297	157
Mean	76	39	74	39
Cycle 3				
	84	37	100	15
	100	3	95	11
	86	33	90	18
	100	40	100	32
Σ	370	113	385	76
Mean	93	28	96	19
Cycle 4				
	100	33	100	—11
	79	36	89	49
	47	—31	95	50
	100	58	100	0
Σ	326	96	384	88
Mean	82	24	96	22
Cycles 1-4				
Mean	81	31	84	23

TABLE 7
RECALL SCORES FOR SUBJECT BEH

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

	Condition A	 B	 C	 D
		Cycle 1		
	5	0	0	0
	4	0	0	0
	3	1	0	0
	5	0	1	0
Σ	17	1	1	0
Mean	4	0	0	0
		Cycle 2		
	4	0	0	0
	3	0	0	0
	5	0	0	0
	7	0	0	0
Σ	19	0	0	0
Mean	5	0	0	0
		Cycle 3		
	7	0	0	0
	0	0	0	0
	7	0	0	0
	6	0	0	0
Σ	20	0	0	0
Mean	5	0	0	0
		Cycle 4		
	7	0	0	0
	7	0	0	0
	5	0	0	0
	10	0	0	0
Σ	29	0	0	0
Mean	7	0	0	0
		Cycles 1-4		
Mean	5	0	0	0

TABLE 8

Recall Scores for Subject Fus

Condition	O.L. List 1	Interpolation	Rel. List 1
A	Waking	Waking rest	Waking
B	Waking	Waking List 2	Waking
C	Waking	Waking rest	Trance
D	Waking	Waking List 2	Trance

	Condition			
	A	B	C	D
	Cycle 1			
	4	0	3	0
	2	0	4	0
	5	0	5	1
	6	2	3	0
Σ	17	2	15	1
Mean	4	1	4	0
	Cycle 2			
	6	0	1	0
	5	0	5	0
	10	0	10	0
	3	0	8	0
Σ	24	0	24	0
Mean	6	0	6	0
	Cycle 3			
	6	1	10	0
	10	0	8	0
	7	1	5	0
	10	0	10	0
Σ	33	2	33	0
Mean	8	1	8	0
	Cycle 4			
	10	0	10	0
	7	0	6	0
	7	0	8	0
	10	1	10	0
Σ	34	1	34	0
Mean	9	0	9	0
	Cycles 1-4			
Mean	7	0	7	0

For both subjects, there appeared to be no reliable difference between Conditions A and C. When the interpolated period was a rest period, the subjects showed no improvement of memory in the trance state. For Beh there was at least a consistent difference between Conditions B and D for each of the 4 cycles, and a large mean difference for all cycles. The saving score was 28 for Condition B as contrasted with a saving score of 2 for Condition D. Hence, instead of obtaining a larger saving score, indicating an improvement of memory, when the relearning was done in the trance, there was a smaller saving score. Fus again showed neither a consistent nor reliable difference between the two conditions.

The results stated in terms of saving in errors are practically identical with those stated in terms of saving in trials as can be seen by comparing Tables 5 and 6 with Tables 3 and 4.

In recall scores, Beh shows a consistent and apparently a reliable difference between Conditions A and C (Table 7). Memory as measured by recall is also less in the trance than in the waking state. A comparison of Conditions B and D shows them practically identical but is quite meaningless because, with one exception, the scores are all zero. The recall scores for Fus show no differences between the waking and trance states (Table 8).

Standard deviations and probable errors were figured for the means given in the tables and reliability of the difference coefficients for all the comparisons made. They are not included because the data were too limited to make them significant.

Conclusions. The results show no improvement in the trance state, as compared with the waking state, for memory of recently learned lists of numbers. This was consistently true for both subjects when judged in terms of saving in trials, saving in errors, and recall. The experiment should be repeated on more subjects who show amnesia for the trance state before any statistically certain conclusions can be drawn.

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L'INHIBITION RÉTROACTIVE ET L'HYPNOSE

(Résumé)

Deux sujets ont appris des listes de dix numéros, chacun ayant trois chiffres, par la méthode d'*anticipation*. On a employé un processus ordinaire d'inhibition rétroactive. La Partie I est un rapport d'une expérience pour tester la théorie de l'hypnose appelée théorie de désassociation. Quand la liste interpolée de numéros a été apprise dans un état (réveil ou transe) différent de celui du premier apprentissage et le réapprentissage, le réapprentissage n'a pas été meilleur. Ainsi la liste interpolée, quoiqu'apprise dans un état différent, n'a pas été complètement désassociée, mais a eu un effet aussi grand ou plus grand sur le réapprentissage qu'une liste apprise dans le même état. La Partie II est un rapport d'une expérience pour comparer la mémoire dans les états de transe et de réveil pour des choses relativement sans signification apprises récemment dans l'état de réveil. Les sujets ont subi 4 cycles, chacun de 16 jours. La mémoire n'a pas été meilleure dans l'état de transe. Les différences qui se sont montrées ont semblé favoriser l'état de réveil, celui où les choses avaient été apprises. Cela s'accorde avec les résultats rapportés dans la Partie I, où la mémoire a été meilleure dans le même état que dans un état différent. A cause du nombre limité des sujets, on devrait répéter les expériences pour obtenir des résultats de valeur statistique.

Mitchell

RÜCKWIRKENDE HEMMUNG UND HYPNOSE

(Referat)

Zwei Versuchspersonen lernten Listen bestehend aus zehn drei-stelligen Zahlen mit der Voraussichtsmethode (anticipation method). Es wurde ein gewöhnliches Verfahren zur Untersuchung der rückwirkenden Hemmung gebraucht: Teil I berichtet über einen Versuch, die Dissoziationstheorie der Hypnose zu prüfen. Wenn die eingeschaltete Zahlenliste in einem Zustande —ob des Wachens oder der Hypnose—gelernt wurde der von dem Zustande des ursprünglichen Lernens oder des Wiederlernens verschieden war, so zeigte sich beim Weiderlernen keine Besserung. Die eingeschaltete Liste, obwohl sie in einem anderen Zustande gelernt worden war, war also nicht völlig dissoziiert, sondern hatte auf das Wiederlernen eine ebenso grosse Einwirkung—oder eine grössere Einwirkung—als eine Liste die in dem selben Zustande gelernt wurde.

Teil II berichtet über ein Experiment zur Vergleichung des Gedächtnisses im hypnotischen und im wachenden Zustand für relativ sinnlose Material welches kurz zuvor im wachenden Zustand gelernt worden war. An jeder Versuchsperson wurde während 4 Perioden bestehend aus je 16 Tagen experimentiert. Es zeigte sich keine Besserung des Gedächtnisses im hypnotischen Zustande. Wo Unterschiede bestanden schienen sie eher den wachenden Zustand zu begünstigen—den Zustand, in dem das Material gelernt worden war. Diese Befunde stimmten mit denen überein über die in Teil I berichtet worden ist, worin ebenfalls das Gedächtniss besser war wenn keine Änderung des Zustandes stattgefunden hatte als bei Änderung des Zustandes. Wegen der Kleinheit der Zahl der Versuchspersonen ist eine Wiederholung der Versuche, um statistisch zuverlässige Befunde zur erhalten, wünschenswert.

MITCHELL

THE RELATIVE INFLUENCE OF ACCOMMODATION AND CONVERGENCE IN THE JUDGMENT OF DISTANCE*

From the Psychological Laboratories of the University of Chicago

H. A. Swenson

Statement of Problem

It is generally admitted that judgments of the distance of visual objects are based to a considerable extent upon the associated processes of convergence and accommodation. The present study is an attempt to evaluate in quantitative terms the relative influence of these two factors in the judgment of distance.

Historical

The problem is one of many years standing, but it remains unsolved in spite of a considerable amount of experimentation. This failure is due to the inadequacy of the apparatus and methods employed.

The experiments in this field fall under two general headings:

1. Comparisons of monocular and binocular judgments.
2. Dissociation of accommodation and convergence.
   a. Dissociation due to inability to accommodate.
   b. Dissociation due to voluntary control of accommodation.
   c. Stereoscopic dissociation.
      1) Free stereoscopy.
      2) Instrumental stereoscopy.

1. The first, or so-called classical method, involves a comparison of the relative accuracy of monocular and binocular judgments of distance. It was employed by Wundt (17), Hillebrand (8), Dixon (7), and Arrer (1), for example. These experimenters attempted to eliminate the influence of all secondary criteria by instrumental means. In general, it was assumed that both convergence and accommodation were present in the binocular adjustments, while only accommodation was operative in the monocular estimations. These experimenters found that the distance of objects could be estimated

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with some degree of accuracy with monocular vision, but that binocular judgments were the more accurate. They concluded that accommodation was effective in so far as accurate judgments were made with monocular vision, and that the convergent factor was responsible for the greater accuracy of the binocular judgments.

The exclusion of all secondary criteria in the classical experiments may be seriously questioned. Furthermore, most of them involve one fatal defect—the fallacious assumption that convergent changes are not present in monocular adjustments to the distance of objects. The presence of convergence in monocular vision is readily observable when vision of the one eye is excluded either by lid closure or by means of a screen. The fallacious character of the assumption was early noted by Arrer (1) and by Judd (12, p. 226), and it was again emphasized by Baird (3) in his admirable critical summary of this literature. The fallacy of the assumption is now generally admitted. Convergence and accommodation are present in monocular as well as in binocular vision. If all secondary criteria are excluded, the ability to judge the distance of objects with monocular vision is mediated by convergence as well as by accommodation, and the greater accuracy of binocular judgments is based upon some other factor—presumably binocular parallax. Obviously, these experiments give no information as to the *relative* effectiveness of accommodation and convergence.

Baird's experiment was modeled after that of Hillebrand. He constructed and employed an improved form of the Hillebrand apparatus and secured and tabulated the data for five subjects for the following five conditions: (1) monocular abrupt series, (2) binocular abrupt series, (3) monocular gradual series, (4) monocular estimation of absolute distance, and (5) binocular estimation of absolute distance.

Baird did not use this tabular data as to the relative accuracy of monocular and binocular judgments in his conclusions concerning the relative influence of convergence and accommodation. As we have already noted, Baird seriously criticized the assumptions of this method in the historical introduction of his paper.

From these circumstances, one may venture the surmise that Baird planned and conducted his experiment in accordance with the classical method, but discovered the fallacy involved in this method during the course of his experiments. This surmise is further supported by the fact that Baird's conclusions as to the relative efficacy of convergence and accommodation were really based upon the existence of pronounced *individual differences* in his data. Obviously, significant data of this sort could not have been anticipated.

2a. Dissociation Due to Defective Accommodation. Baird's conclusions are based upon the fact that one of his subjects, B, gave results which were different from those of his other subjects, and an ocular examination showed that this subject had defective accommodatory ability. Hence Baird's experiment is to be classed under the above rubric.

Baird assumed that all secondary criteria were excluded by his apparatus. This assumption might be seriously questioned, but we shall grant it for the purposes of this paper. It follows that accommodation and convergence are the only possible factors in the monocular judgments, while the additional factor of binocular parallax may be present in the binocular judgments.

We quote the following conclusions of Baird (3, p. 47):

> "It has also been shown that in monocular vision—when the factors present are reduced to accommodation and convergence —the presence of normal accommodation has been attended by the capacity accurately to estimate relative distances, while the absence of normal accommodation has been paralleled by the absence of this capacity. From this we conclude that *accommodation constitutes the essential criterion of depth in our monocular experiments.*
>
> "It follows from our binocular results that the essential criterion in the binocular experiments was not furnished by accommodation."

It may be noted that Baird does not specify the other factor operative in his binocular experiments. Obviously, it would need to be either convergence or binocular parallax.

The statement as to the efficiency of accommodation is stated in general terms, i.e., it is applicable to all subjects, and not limited to Subject B with defective accommodation.

Baird gives his tabular data for each of the five subjects and for each of his five experimental conditions; but he is nowhere clear and definite as to the precise data upon which his conclusions are based. Certainly not all of his comparative data are significant for his conclusions. We shall thus state and discuss the significance of the factual data of his tables.

1. Subject B's judgments in the monocular abrupt series were a

failure, but he exhibited normal accuracy in the monocular gradual series and in the monocular estimations of absolute distance.

Apparently, Baird assumed that this subject was unable to accommodate in the monocular abrupt series, but that he was able to do so in the other two conditions. On these assumptions, it might be concluded that monocular judgments of distance are based upon the factor of accommodation, while convergence is not effective. It may be noted, however, that this conclusion, based upon these data, is strictly applicable only to Subject B.

Another assumption is possible. The subject was unable to accommodate in the monocular abrupt series. Since accommodation and convergence are associated processes, we may make the additional assumption that convergent changes were also absent in this condition. Likewise, *both* accommodation and convergence were present *in the other two conditions.* On this assumption, monocular judgments of distance for Subject B are based upon accommodation and convergence, and no statement can be made as to their relative efficacy. We are not defending this assumption; we merely mention it as a possibility for consideration.

2. Subject B was unable to make any judgments of distance in the monocular abrupt series, but he exhibited normal accuracy in the binocular abrupt series.

This fact is apparently explained on the assumption that accommodation is absent in the first condition and present in the second, while convergence is present in both. Accommodation is thus regarded as the effective factor, while convergence has no effect. But again this conclusion can refer only to Subject B.

This conclusion ignores the further consideration that binocular parallax is absent in the first condition but is present in the second. This fact, taken by itself, is just as indicative of the functional efficacy of binocular parallax. When the fact is considered in conjunction with the previous data, it definitely supports the accommodatory hypothesis.

However, one can assume the absence of both accommodation and convergence in the monocular abrupt series. On this basis, both factors must be regarded as efficacious.

3. Subject B's binocular estimations of absolute distance were more accurate than his monocular estimates.

This fact is not commented on, but presumably the greater binocular accuracy is due to binocular parallax.

4. The comparative records of the normal subjects for the five conditions are obviously not significant for our purposes. These represent the standard type of data secured by Wundt, Hillebrand, and Dixon by the use of the classical method, and Baird has agreed that such data throw no light upon the problem of the relative efficacy of convergence and accommodation; rather they prove the efficacy of binocular parallax.

5. There remains for consideration only a comparison of the records of Subject B with those of the normal subjects for each of the five conditions.

Subject B was unable to make any judgments as to distance in the monocular abrupt series, but the normal subjects could. For all other conditions, Subject B compared favorably with the normal subjects. In only one condition was Subject B consistently the poorest subject of the group, and that was the case in which the subject was required to judge *the direction* of movement in the monocular gradual series.

Presumably these comparative data are to be explained in terms of the absence and presence of accommodation.

Two general comments may be made in regard to Baird's conclusions:

1. The conclusions are based primarily upon the inability of Subject B to make any judgments in the monocular abrupt series and the correlation of this fact with the assumed absence of accommodation under these conditions. One feels indisposed to settle the question upon such limited data. It is entirely possible that the relative influence of accommodation and convergence may vary with individuals. Certainly it is unwise to generalize on the basis of a single unique and inconsistent case.

2. Let us grant Baird's conclusion that accommodation is the most important factor in monocular judgments. We have been unable to find any empirical justification for his second conclusion that accommodation is not the most important factor in the binocular judgments.

2b. Voluntary Control of Accommodation. Another form of experimentation has involved the rather unusual ability of dissociating accommodation and convergence by voluntarily controlling accommodation. Several such cases have been described by Professor Carr (4, 5). A typical case is that of Miss Allen (6). This subject possesses a direct, voluntary control of accommodation independent of the convergence. Depth is in her case a function of accommodation and is in no respect effectively influenced by changes in convergence.

Miss Allen observed two wires of identical size and appearance held in a vertical position and parallel to one another in a screw device a few inches in front of her eyes. She combined the foveal images of the two wires stereoscopically and then voluntarily controlled the depth location of the combined image by varying accommodation. Not only was Miss Allen able to observe changes in the apparent location of the combined image, but she perceived movement as well when the eyes were held in a fixed convergent position and accommodation voluntarily altered. Accommodation could also be held constant when convergence was altered by means of the screw device and the combined image was perceived constantly at some prescribed location.

The authors made no attempt to generalize on the basis of this single unusual case. The ability to voluntarily control accommodation in the manner described above is comparatively rare. Our problem concerns itself with the usual, normal functioning of accommodation and convergence in the perception of depth, and we are interested in special cases only in so far as they throw light on the wide variability among individuals with regard to the relative influence of the two factors.

2c. *Stereoscopic Dissociation.* A third method of dissociating accommodation and convergence is that of stereoscopic dissociation which falls into two classes, namely, "free" stereoscopy, represented by Le Conte (15) and Hyslop (10), and instrumental stereoscopy (13, 14).

1) *Free stereoscopy.* Hyslop possessed a high degree of proficiency in the art of free stereoscopy. He voluntarily converged in front of or behind the plane of two similar figures or objects, and binocularly combined the images of these objects. The lens failed to follow the convergent changes and remained accommodated for the actual distance of the object, for the two images were seen distinctly. The stereoscopic image was apparently located near the intersection of the visual axes rather than in the plane of accommodation. Depth was thus mainly a function of convergence rather than of accommodation. One cannot conclude that accommodation was not effective, nor can one make any exact quantitative statement as to their relative efficacy, except that convergence was much more effective. Secondary criteria were not excluded in these conditions,

and one does not know with certainty that the stereoscopic image was located exactly at the point of convergence rather than a short distance from it.

This case is the exact opposite of that of Miss Allen. For Hyslop, depth was mainly a function of convergence. For Miss Allen, depth is primarily a function of accommodation, the process which in her case possesses the most volitional importance. These cases represent opposite extremes, and we again note the probability that the relative influence of the two factors may be a wide individual variant. Both represent unusual cases from the standpoint of volitional dissociation and one must be cautious in generalizing from these to normal cases.

2) *Instrumental stereoscopy.* The mirror type of stereoscope is usually employed in these experiments. The two stereograms to be combined are placed at the ends of the two arms, and the light from these is reflected into the eyes by means of two small mirrors placed before the eyes. The stereoscopic image is then seen in front of the eyes at some distance beyond the mirrors.

Convergence can be altered in several ways: by rotating the two arms in relation to the mirrors, by changing the angle of the mirrors in relation to the eyes, by altering the distance between the two mirrors, and by moving the mirrors backward and forward in relation to the eyes. The stereoscopic image can be observed to move backward and forward with these induced changes of convergence. This experiment proves that convergence is a factor in the perception of distance, but one can make no definite quantitative statement as to its efficacy relative to that of accommodation for two reasons:

1. One does not know the exact location of the stereoscopic object in relation to the intersection of the visual axes. One does not know that the location of the stereoscopic object varies proportionally with convergence.

2. As Judd has pointed out (13, p. 388), all of these adjustments of the apparatus whereby convergence is altered also change to some extent the distance of the stereograms from the eyes. Consequently, accommodatory changes are involved as well as such secondary factors as intensity and size of the retinal image. Convergence is not altered with all other factors held constant. The perceptible change of distance may thus be due in part to these other factors.

Judd performed an experiment which is practically free from the second objection. In place of the stereograms, he used a star as the fixation object. He adjusted the mirrors so that the star was reflected from both mirrors. By changing the angle of the mirrors to each other, the convergence was altered and the stereoscopic image of the star moved back and forth with the convergence. Any possible change of the distance between the eyes and the star was so small as to be negligible. For all practical purposes only convergence was altered, and these changes were quite effective upon the perception of distance.

The mirror stereoscope is still more defective in isolating accommodation. For this purpose it is customary to move the two stereograms along the arms nearer to or farther away from the mirrors and hence in relation to the eyes. The stereoscopic object may be observed to move backward and forward under these circumstances. Accommodation is altered in these adjustments while convergence is kept constant. However, one cannot conclude that the observed movement of the stereoscopic object is due to the accommodatory changes, for the size and intensity of the retinal stimulus is also altered proportionally. The ordinary mirror stereoscope involves insuperable defects so far as the isolation of accommodation is concerned.

In our experiments we employed a modified form of the mirror stereoscope which was so designed as to eliminate the defects we have noted in the usual form of the apparatus. Our stereoscope permits the alteration of either convergence or accommodation while keeping the other constant and without introducing any concomitant changes of any of the secondary criteria. The apparatus also involves a device which permits some quantitative statement of the location of the stereoscopic image in relation to the intersection of the optic axes and to the plane of accommodation. As previously noted, previous experiments with the stereoscopic method were defective in this respect.

Apparatus

The apparatus we used was a new form of stereoscope designed and constructed under the direction of Professor Harvey Carr. A rough working model was first built and on the basis of this model the present stereoscope was perfected. It consists essentially of two brass telescopic tubes supported by a metal stand and fitted at the far ends with translucent screens of milk glass behind which are attached 2 ½-volt flashlight bulbs fed by dry batteries and controlled

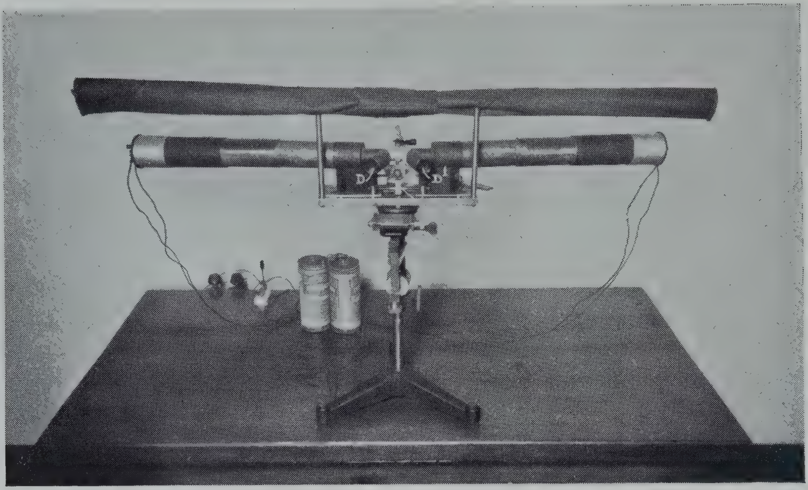

FIGURE 1

by rheostats. The tubes are bent at an angle of 88 degrees near the fixation ends and are fitted with mirrors at the angles so that when the subject is looking into the tubes the visible areas of the translucent screen at *A* and *A'* are seen as a single circular light at some point back of the mirrors in the plane of regard. (See Figure 1.) The mirrors are silvered on the front side rather than on the back to insure a perfect reflecting surface.

The control of convergence without altering the distance of the stereoscopic objects—the visible areas of the luminous screens—was accomplished in the following manner. Each telescopic tube was mounted upon a carriage which moved in a circular slot whose center coincided with the center of rotation of the corresponding eyeball when the head was in the proper observation position in the headrest. By means of a knurled wheel (*B*) the telescopic tubes were moved in the circular slots (*C* and *C'*) and consequently the convergence of the eyes was altered. Since the tubes and the eyes have the same center of rotation, it is obvious that the distance between the eyes and the luminous screens must remain constant during these convergent rotations. With constant distance there can be no concomitant changes of accommodation or changes in the size and intensity of the retinal image.

A metal disc is located in each tube at the positions *D* and *D'*.

In the center of each disc is a small knife-edge opening 1/16 of an inch in diameter. These openings are just large enough to permit an unobstructed view of the luminous screens when the eyes are in the proper position and the apparatus has been adjusted to the correct interocular distance. A small turnscrew at E makes this interocular adjustment possible. When the tubes are rotated, the luminous screens will continue to be visible if the centers of eye rotation coincide with the centers of rotation of the tubes. If the two centers of rotation differ to any marked extent, the presence of this small opening will prevent the subject from seeing the stereoscopic objects as whole and circular throughout the entire extent of convergent rotation. The ability of the subject to see the luminous circular object through the convergent adjustments thus indicates that the eyes have been properly adjusted to the apparatus.

Acommodatory changes were induced in the following manner. The distant ends of the tubes were of the telescopic or collapsible variety, and hence the distance of the stereoscopic objects—the luminous screens—from the eyes could be varied at will. Accommodation was thus altered without affecting the degree of convergence.

Concomitant changes in the size and intensity of the retinal images were excluded by the following device. A metal disc containing a knife-edge opening with a diameter of 7/16 of an inch was located at F and F' in the figure. This size of opening did not permit a full view of the luminous screens even when the tubes were extended to their full length. The part of the tubes between F and F' and the eyes was not collapsible, and hence the distance between these openings and the eyes remains constant. As the luminous screens are moved toward the eyes, the surface area that is visible is decreased proportionally with the distance. In other words, the angular extent of the surface that is visible remains constant—that is, the retinal image does not vary in size.

Likewise, proportionally less light from the illuminated screens enters the eyes because less of the screens is visible. Consequently, the intensity of the retinal image likewise remains constant. The formula is $I = \frac{kai}{d^2}$, where I is the total intensity, k is a constant, a is the area, i the intensity per unit area, and d the distance from the eye to the source of light.

To keep the intensity of the retinal light constant as the distance of the screens from the eyes is varied, reflected light from the sides of the tubes must not be permitted to enter the eyes. This was done in the usual way. The interior of the tubes was painted an optical black and a series of other metal discs with graduated openings were placed at intervals within the tubes between *D* and *F* and *D′* and *F′*.

The constancy of the intensity of the retinal light was easily tested in the following manner. The two tubes were so adjusted (by means of the knurled wheel *B*) that the binocular images were not combined. Consequently, the subject saw the two lights closely adjacent to each other. The two lights were adjusted by means of the rheostats until they were judged to be equal in intensity. The distance of one of the lights was then altered and the subject was again asked to judge their relative intensity. None of the subjects was able to detect any intensity changes in the altered light. The test proves that the intensity of the retinal light as judged by the subjects remains constant for all positions of accommodation.

The following method was employed to secure quantitative judgments as to the location of the stereoscopic object in relation to the plane of accommodation and the intersection of the visual axes. A brass rod 60 cm. in length projected straight back from the apparatus along the line of vision. It was calibrated in centimeters with the position of the eyes as the zero point of reference. To this was attached a movable pointer (*G*) which could be moved back and forth along the rod by means of a small wooden handle that could be conveniently grasped by the subject. The collapsible tubes were also calibrated in centimeters with the position of the eyes as the zero point of reference. The stereoscopic objects could thus be set at any desired distance from the eyes. The wheel (*B*) controlling the rotation of the tubes and the convergent status of the eyes was carefully calibrated by empirical means so as to indicate the distance of the intersection of the visual axes for the various adjustments. By means of these calibrations, the experimenter could easily and readily adjust either the accommodation or the convergence for any desired distance from the eyes.

Let us suppose that the accommodation is set for 25 cm. and the convergence for 40 cm. The subject then places his head in the required position and observes the stereoscopic image. He is then instructed to grasp the handle of the pointer and move it along the calibrated rod to the point where the visual object appears to be located. The experimenter then records the position of the pointer in terms of its calibration which indicates the apparent distance of the visual object from the observer's eyes. If the reading were 36 cm., it would indicate that the subject perceived the stereoscopic object nearer to the point of convergence than to the plane of accommodation.

This motor method of measuring the apparent distance of a stereoscopic object was borrowed from Professor Ashley (2) who successfully employed a somewhat similar technique. Obviously, the accuracy and reliability of such a method cannot be judged on *a priori* grounds. Its validity can be tested only in terms of the accuracy and the reliability of the measures obtained with its use. As we shall see, the method furnished results of greater accuracy and reliability than one would expect.

The stereoscope was mounted on an adjustable upright bar, permitting the raising or lowering of the instrument to any desired height, with a heavy metal base firmly screwed to a substantial table. A three-way adjustable mouth-bit head-rest (*H*) attached to the upright bar insured steadiness of vision. A black curtain, leaving only the eye-holes and the head-rest visible to the subject, covered the entire apparatus.

PROCEDURE

In the first set of experiments five subjects were employed. Three were graduate students in psychology and two were undergraduates taking work in the Department. The subject seated himself before the instrument which was then raised or lowered to a convenient height. The tubes were set at the proper interocular distance and the head-rest adjusted so that when looking into the tubes the subject could steadily observe the single circular area of light, like a small white moon of low intensity, apparently some distance straight back along the line of vision. An intensity of light was so chosen, and kept as constant as possible throughout the experiments, that it would be clearly visible yet not too fatiguing to the eyes.

All the subjects knew in a general way that the experiment was designed to test accuracy of visual localization, but were ignorant of the mechanical nature of the apparatus. They were merely instructed to fixate the light and to move the marker, as accurately as possible, to the apparent location of the light back of the screen. Introspections were called for from time to time in an attempt to throw some light upon the nature of the cues on which their judgments were based.

A number of preliminary judgments was made as a practice series to acquaint the subject with his task; but at no time during the preliminary practice or the entire experimental series was any indication of the accuracy or inaccuracy of his judgments given the subject.

Three positions of convergence and three corresponding positions of accommodation were arbitrarily selected as standards which, in combination, gave a total of nine conditions, namely: (1) accommodation and convergence both at 25 cm.; (2) accommodation and convergence at 30 cm.; (3) accommodation and convergence at 40 cm., the three conditions selected as standards; (4) accommodation at 30 cm., convergence at 25 cm.; (5) accommodation at 25 cm., convergence at 30 cm.; (6) accommodation at 30 cm., convergence at 40 cm., (7) accommodation at 40 cm., convergence at 30 cm.; (8) accommodation at 25 cm., convergence at 40 cm.; and (9) accommodation at 40 cm., convergence at 25 cm.

The several conditions were presented in random order until 120 or more judgments had been recorded for each of the nine conditions, or a total of at least 1080 judgments for all conditions for each of the five subjects in the first set of experiments.

When the experiment was ready to proceed, a condition was selected and set by the experimenter. The subject then looked into the stereoscope, fixated the light, and moved the marker *out* as deliberately as he chose to its apparent location back of the screen. On the next judgment, for the same or a changed condition, the subject was required to move the marker *in* from a more distant point along the brass rod. Thus, for each subject, an equal number of out-going and in-coming judgments was recorded for each condition. On the whole, the former proved to be slightly more accurate. The shifts in the conditions were always made at a time when the subject was not observing the light. At the point where the subject reported the first signs of eye-strain the experiment was discontinued until the following or a later day. Usually not more than 15 or 20 judgments were made at a single sitting.

RESULTS AND CONCLUSIONS

Table 1 shows the means and standard deviations, based on 120 to 240 judgments in each condition, for the three standard conditions for each of the five subjects, and the average means for the sum total of judgments of all the subjects for the three standard conditions.

TABLE 1

MEANS AND SIGMAS OF STANDARD CONDITIONS EXPRESSED IN CENTIMETERS

	I		II		III	
	A 25		A 30		A 40	
	C 25		C 30		C 40	
Subjects	Mean*		Mean		Mean	
A	24.62	1.52	29.78	2.00	39.50	1.79
B	25.33	1.16	30.28	1.43	39.48	1.59
C	24.04	1.53	29.90	1.84	40.36	1.75
D	25.05	1.35	29.89	2.01	39.87	1.59
E	24.14	1.38	29.72	2.05	39.83	1.74
All subjects	24.80		29.88		39.81	

*The mean values are based on 120 to 240 judgments.

All subjects showed a high degree of accuracy in their judgments in the standard conditions. The reader will recall that the subject is required to react motorly to what is apparently a single, undifferentiated circle of light "somewhere out there" before his eyes; that he has absolutely no other object in the visual field with which to relate the fixated light; and, further, that he must localize the light by means of an unseen movement.

It is evident from the data of Table 1 that the judgments of the distance of the stereoscopic object were extremely accurate. The deviations of the mean values from the true distance were always less than 1 cm. In the majority of cases the mean values were slightly less than the "true" distance. The judgments of any individual were relatively quite consistent. The sigma values for any group of judgments ranged from 1.16 to 2.05 cm.

These facts prove that the associated factors of convergence and accommodation are highly effective in mediating judgments of distance within the range of 25 to 40 cm. They also demonstrate the validity of our motor method of quantifying these judgments. It is a curious fact, but by no means new in experimental findings, that even in those occasional judgments where the subject reported a "lost" feeling, where he felt his judgments to be hardly more than mere guesses, there was usually little or no loss in accuracy. Our judgments are often based on unconscious cues; we react successfully, we know not how.

Table 2 shows the average judgments for all combinations of convergence and accommodation, including the three standard conditions separately shown in Table 1. The standard deviations are not in-

TABLE 2

Showing Average Judgments for All Conditions in Terms of Centimeters

Subjects	I A C 25 Mean	II A C 30 Mean	III A C 40 Mean	IV A 30 C 25 Mean	V A 25 C 30 Mean	VI A 30 C 40 Mean	VII A 40 C 30 Mean	VIII A 25 C 40 Mean	IX A 40 C 25 Mean
A	24.62	29.78	39.50	26.91	29.17	37.83	32.00	37.23	28.10
B	25.33	30.28	39.48	26.84	28.13	36.46	32.36	36.43	27.13
C	24.04	29.90	40.36	26.40	27.47	36.63	31.96	37.22	25.80
D	25.05	29.89	39.87	25.92	29.21	35.98	31.89	36.86	28.34
E	24.14	29.72	39.83	26.49	28.70	34.89	30.81	36.99	28.33
Averages for all subjects	24.80	29.88	39.81	26.51	28.55	36.36	31.74	36.95	27.54

cluded in Table 2 inasmuch as an examination and comparison of the data for Conditions IV to IX indicated that the standard deviations for these conditions were comparable to those for the standard conditions. This was borne out by the computation of the standard deviations for a number of these conditions chosen at random.

Two general facts may be drawn from the data of Table 2.

1. When accommodation and convergence are dissociated and adjusted for two different distances, e.g., 25 and 30 cm., the stereoscopic object is invariably located at a position intermediate between those obtained when both accommodation and convergence were adjusted for 25 and 30 cm. For example, when both convergence and accommodation were at 25 cm., the mean judgment for Subject A was 24.62 cm. When accommodation and convergence were at 30 cm., the mean value of the judgments was 29.78 cm. When the two were dissociated and one was at 25 and the other at 30 cm., the mean values were 26.91 and 29.17—both being intermediate between 24.62 and 29.78. There was no exception to this rule. This fact indicates that both accommodation and convergence were effective upon the judgments of distance when the two were dissociated.

2. It may also be noted in the above illustration that, in the case of dissociation, the stereoscopic object is located nearer to the point of convergence than to the plane of accommodation. Again there is no exception to this rule, and this general fact proves that convergence is more effective than accommodation in these judgments of distance.

The relative influence of accommodation and convergence in these judgments is expressed in percentage terms in Table 3. These percentage values were derived from the data of Table 2 in the following manner: The mean value for Subject A was 24.62 cm. when accommodation and convergence were both set at 25 cm. The mean value was 29.78 cm. when the two were set at 30 cm. With convergence at 25 cm. and accommodation at 30 cm., the mean value was 26.91 cm. The difference between 26.91 and 29.78 is 2.87, and this value may be taken as a measure of the influence of convergence. Likewise, the difference between 26.91 and 24.62 is 2.29, and this is regarded as the measure of the influence of accommodation. The relative effectiveness of convergence and accommodation is thus expressed by the values of 2.87 and 2.29, respectively. All such values were then reduced to a common denominator by expressing them in percentage terms. The corresponding percentage values are 56 and 44, and these values are entered for Subject A, Condition IV, in Table 3.

An examination of Table 3 suggests the following conclusions:

1. The data of Table 3 again support our previous observation that both accommodation and convergence were effective in every case, for it will be noted that no zero values appear.
2. Convergence was always a more effective factor than accommodation as is evident from the fact that the percentage values for convergence are higher in every case. On the general average, convergence was found to be about three times as effective as accommodation in the judgment of distance within the limits of our experiment.
3. There is no conclusive evidence that the relative effectiveness of convergence and accommodation varies with individuals. The individual range of average values for convergence is from 72 to 79 per cent. However, the number of subjects was limited.
4. The relative effectiveness of the two factors varies with the degree of dissociation. When widely dissociated there is apparently a tendency to ignore accommodatory cues and place more reliance upon those of convergence.

EFFECT OF SIZE OF APERTURE

A very small aperture, e.g., a pin-hole, serves to give a clear image regardless of the degree of accommodation (9, 16). Such an aperture thus makes accommodatory changes unnecessary. In view of this fact, the question was raised as to whether the small size of the eye-holes used did not materially diminish the amount of accommo-

TABLE 3*

PERCENTAGES SHOWING RELATIVE INFLUENCE OF ACCOMMODATION AND CONVERGENCE

	I A 25 C 25 II A 30 C 30 III A 40 C 40	IV A 30 C 25		V A 25 C 30		VI A 30 C 40		VII A 40 C 30		VIII A 25 C 40		IX A 40 C 25		Average percentages	
Subjects	Standards	A	C	A	C	A	C	A	C	A	C	A	C	A	C
A	"	44	56	12	88	17	83	23	77	15	85	23	77	22	78
B	"	31	69	43	57	33	67	23	77	22	78	13	87	27	73
C	"	40	60	42	58	36	64	20	80	19	81	11	89	28	72
D	"	15	85	14	86	39	61	20	80	20	80	22	78	21	79
E	"	42	58	18	82	49	51	11	89	18	82	27	73	27	73
Averages		34	66	26	74	35	65	19	81	19	81	19	81	25	75

*The first line indicates the various conditions of the experiment. The second line describes the several conditions. *A 25* means that accommodation was set at 25 cm. *C 25* means that convergence was set at 25 cm. The third line indicates the percentage of contribution that accommodation and convergence made to the actual judgments of the subjects. The following lines give these percentages.

TABLE 4*
PERCENTAGES SHOWING RELATIVE INFLUENCE OF ACCOMMODATION AND CONVERGENCE
(Larger eye-hole)

I A 40 C	II A 30 C	III A 25 C	IV A 30 C 25		V A 25 C 30		VI A 30 C 40		VII A 40 C 30		VIII A 25 C 40		IX A 40 C 25		Average percentages	
Subjects	Standards		A	C	A	C	A	C	A	C	A	C	A	C	A	C
C	"		38	62	39	61	26	74	24	76	20	80	22	78	26	74
F	"		38	62	32	68	29	71	23	77	18	82	16	84	23	77
G	"		29	71	31	69	51	49	20	80	26	74	21	79	27	73
H	"		39	61	39	61	23	77	29	71	18	82	24	76	26	74
Averages			38	62	38	62	29	71	23	77	20	80	20	80	25	75

*The first line indicates the various conditions of the experiment. The second line describes the several conditions. *A 25* means that accommodation was set at 25 cm., etc. The third line indicates the percentage of contribution that accommodation and convergence made to the actual judgments of the subjects. The following lines give these percentages.

dation necessary for distinct vision, and hence minimize the relative influence of this factor.

To test this possibility, we increased the size of the eye-holes from 1/16 of an inch (the original size) to 5/16 of inch and repeated the experiment under otherwise similar conditions. This size of aperture exceeds the average size of the pupil in the normal, light-adapted eye. All observations were made under daylight conditions.

Four subjects were employed in this test which was conducted in the following year. Only one of these (Subject C) had also served in the first experiment. All the subjects in the test were graduate students in psychology.

Table 4 was derived in exactly the same manner as Table 3, and it presents the relative influence of convergence and accommodation, under the conditions of the larger eye-hole, in terms of percentages. On the basis of the comparative data of Tables 3 and 4 the following factual statements may be made:

1. The values for accommodation are larger in Table 4 for five of the six conditions for the group averages, but the differences are slight and probably are not statistically significant. However, the composition of the two groups is different with the exception of one subject.

2. Only one subject was employed in both experiments, namely, Subject C. In this case, the values for accommodation were slightly less for the larger opening, and the differences are not statistically significant.

There is thus no conclusive evidence that the effectiveness of accommodation increases with the size of the opening, and we may conclude that accommodation possesses only about one-third of the effectiveness of convergence for the distance limits which we investigated.

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L'INFLUENCE RELATIVE DE L'ACCOMMODATION ET DE LA CONVERGENCE DANS LE JUGEMENT DE LA DISTANCE

(Résumé)

Le but de cette étude a été d'évaluer, en termes quantitatifs, l'influence relative de l'accommodation et de la convergence dans le jugement de la distance. On a employé une nouvelle forme de stéréoscope à miroir au moyen duquel il a été possible de varier la convergence pendant que l'accommodation, la grandeur de l'image rétinale, l'intensité de l'objet stéréoscopique et tous les autres critères secondaires de la distance visuelle sont restés constants. On a pu aussi varier l'accommodation pendant que la convergence, l'intensité, etc., sont restées constantes. On a employé une méthode motrice pour mesurer la distance apparente de l'objet stéréoscopique. Quand la machine a été placée pour une des différentes combinasons de convergence et d'accommodation (on a choisi arbitrairement neuf conditions, dont on a choisi trois comme contrôles), le sujet a fait mouvoir un indicateur d'un côté ou de l'autre sur un bâton de laiton étalonné à la position apparente de l'objet stéréoscopique. On a employé cinq sujets. Tous ont montré un haut degré de précision dans les trois conditions de contrôle. Les résultats dans

les conditions où la convergence et l'accommodation ont été désassociées ont suggéré les conclusions suivantes: (1) Que l'accommodation et la convergence toutes deux ont opéré dans tous les jugements; (2) Qu'en moyenne générale, la convergence a eu trois fois l'effet de l'accommodation dans les limites de l'expérience (c'est-à-dire, de 25 à 40 cm.); (3) Que nulles différences individuelles marquées n'ont été apparentes; (4) Que l'effet relatif des deux facteurs a varié avec le degré de désassociation.

SWENSON

DIE RELATIVE EINWIRKUNG DER ANPASSUNG UND DER KONVERGENZ BEI DER BEURTEILUNG DER ENTFERNUNG

(Referat)

Es war das Ziel dieser Untersuchung, die relative Einwirkung der Anpassung der Konvergenz bei der Beurteilung der Entfernung quantitativ zu bewerten. Es wurde Gebrauch gemacht von einer neuen Form des Spiegelstereoskops (mirror stereoscope) mit dem es möglich war, die Konvergenz zu variiren während Anpassung, Grösse der Netzhautabbildung (retinal image), Intensität des stereoskopischen Gegenstandes, und alle übrige sekundäre unterscheidende Merkmale der visuellen Entfernung konstant gehalten wurden. Ebenfalls konnte die Anpassung variirt werden während Konvergenz, Intensität, u.s.w. konstant blieben. Es wurde eine motorische Methode zur Messung der scheinbaren Entfernung des stereoskopischen Gegenstandes gebraucht. Wenn die Maschine für eine der verscheidenen Kombinationen von Konvergenz und Anpassung geregelt worden war (es wurden neun Zusammensetzungen willkürlich ausgewählt, von denen drei dann als Massstabe dienten) bewegte die Vp. einen Zeiger auswährts (oder nach innen) einen kalibrierten Stab aus Messing entlang, bis zur scheinbaren Lage des stereoskopischen Gegenstandes. Es wurden 5 Vpp. in Anspruch genommen. Alle erwiesen unter den drei massgebenden Gruppen von Umständen einen hohen Grad der Genauigkeit. Die Befunde unter den Umständen in denen Konvergenz und Anpassung dissoziert waren, weisen auf folgende Schlüsse hin: (1) Dass sowohl Anpassung wie Konvergenz bei allen Urteilen wirksam waren; (2) dass, im allgemeinen Durchschnitt, die Konvergenz innerhalb der Grenzen des Versuchs (d.h., 25 bis 40 cm.) ungefähr drei Mal so stark wirkte wie die Anpassung: (3) dass sich keine ausgesprochene individuelle Unterschiede zeigten; (4) dass die relative Wirksamkeit der zwei Einwirkungen mit dem Grad der Dissoziation variirte.

SWENSON

AN EXPERIMENTAL COMPARISON OF ADVERB-VERB AND VERB-ADVERB CONTROLLED ASSOCIATION REACTION TIMES*

From the Psychological Laboratory of The Johns Hopkins University

Laurence A. Petran

A study of the effect of language factors on association reaction times is important for both general and applied psychology. The hypotheses that the various mental processes commonly grouped under the name of thinking are largely dependent on or entirely a matter of implicit verbalization have become widely prevalent in psychology and have been exploited in one form or another by the behaviorists. The association test is now a widely accepted and somewhat standardized tool for the investigation both quantitatively and qualitatively of mental processes. If it is assumed that the mental processes of a reactor after the presentation to him of a stimulus word are similar in important respects to the rest of his mental activity, if not necessarily representative of all of it, then studies of the association reaction, especially of the verbal association reaction, will be admitted to have a significant bearing on all hypotheses concerning thinking. The association test has also been extensively applied in psychiatric analysis and for the purpose of detecting guilty knowledge, and in both instances the length of the association times is of importance. Obviously, before the association reaction times of a neurotic or a suspect can be called of indicative length, there must be known and recognized how much variation in the association reaction times of normal and innocent reactors is due to the particular parts of speech used as stimulus words.

Investigators are generally agreed that the association reaction time to an adjective stimulus is substantially shorter than that to a noun stimulus, whether the association is free or the response word is limited to nouns for adjective stimuli. A large proportion of the words in the more widely used stimulus lists are nouns and adjectives. There is less unanimity as to the association reaction times to adverb

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and verb stimuli, except that they are consistently longer than the association reaction times to adjective stimuli, and much less study has been made of them. This study is limited to a comparison of controlled association reaction times to adverb and verb stimuli, when verb and adverb responses respectively are called for. It is a repetition of two experiments of Finesinger (5), the second of which he was unable to complete.

The history of the association method has been reviewed comprehensively by both Finesinger (5) and Loring (6) in their doctoral dissertations. It has also been covered rather completely by Crosland (2) with special reference to its use in criminal procedure, and by Miles and Terman (7), who have summarized the findings of association tests with respect to sex differences.

With the use of the Hopkins chronoscope (3) and Dunlap voice keys (4) several extensive studies of the association reaction have been made at the Hopkins psychology laboratory. Loring (6), in order to avoid the pitfalls inherent in the arbitrary making-up of stimulus lists and the classification of responses according to logical schemes, made a survey of a standard dictionary, selected 10,888 nouns, adjectives, and verbs, and limited her work to controlled association using grammatical categories. She found that for controlled association the adjective-noun response was shorter than the noun-adjective, and the verb-object shorter than the verb-subject. This is in accordance with what would be expected from the normal English word order. She found also that the reaction time increased with the number of syllables in the stimulus word, but that no effect on the reaction time could be traced to accent or to position of the word within a logical category. Successive repetitions of a list of stimulus words without forewarning the reactor caused successive decreases in the average reaction times.

Finesinger (5) in his unpublished dissertation reported a study of controlled association reactions to numbers and words with university students as reactors. To nine reactors he presented as stimuli the numbers from 1 to 50, five even numbers alternated with five odd, but otherwise in chance orders for ten days or so. In alternate periods the reactors responded by adding one to the stimulus number or substracting one from it. Six reactors had the average reaction time for addition shorter than that for subtraction, and two had the reverse. This he attributes to the greater frequency of use of addition in daily life. In addition, the average reaction time to odd stimulus numbers was longer than that to even numbers in 82% of the cases; in subtraction the same was true in 76% of the cases. He confirmed Loring's results as to the comparative speed of the adjective-noun and noun-adjective responses and found that this relation held in 89% of the cases with eight reactors throughout ten days' work with the same stimulus lists repeated from day to day. Using dissyllabic verbs and adverbs as stimulus words, requiring verb responses to adverbs and adverb responses to verbs, and repeating the same lists for ten days with six reactors, nine days with one, and seven days with one, he found the average verb-adverb time to be the longer in 79% of the cases, contrary to what would be expected *a priori* from the usual English word order. Sentences written by the reactors using each stimulus word and its most frequent response word showed a word order that was predominantly verb-adverb in seven out of the eight reactors. To explain the slower speed of the more frequently used word order, he quotes the late Professor Bright of Johns Hopkins University to the effect that the verb is a more important unit of language than the adverb, and that adverbs are merely attributes. Inasmuch as all the dissyllabic adverbs had *-ly* for the second syllable, a further experiment with trisyllabic adverbs and a new list of dissyllabic verbs as stimulus words was projected, but was not carried beyond the initial stages.

This method of repeating the same lists of stimulus words on successive days was first used by Finesinger at Dr. Dunlap's suggestion as a means of obtaining a sufficient number of responses without employing an extensive list of words. Particularly with adverbs, extending the lists necessarily introduces words of widely varying familiarity and difficulty, largely defeating the very purpose for which extensive lists would be employed. There seems to be distinct advantage in the daily repetition method, especially where it is desired to arrive at the mechanized speech function on which most association work bears.

Browning (1) made an intensive study of association processes with six reactors, three boys and three girls, who were high-school seniors. In one of the experiments he presented lists of 25 words each, nouns, adjectives, verbs, and adverbs from the Loring lists in the above order and repeated the lists with the order of words within the list shuffled, at intervals of a day or so for five days. He found that responses to adverbs and verbs showed less regularity and less variety than those to adjectives and nouns. There was a tendency for adverb and verb responses to be less rapid than adjective and noun responses. Half the reactors tended to associate adverbs to verbs more rapidly than verbs to adverbs, and the other half showed the reverse tendency.

Experiments I and II

The two experiments reported here are identical throughout as to apparatus, reactors, and procedure; but in Experiment I dissyllabic adverbs and verbs were used as stimulus words, and in Experiment II trisyllabic adverbs and dissyllabic verbs were the stimulus words.

The set-up of the apparatus is shown in Figure 1. The chronoscope was activated by a 6-volt, 62.5-cycle current delivered through a Tungar rectifier from the 110-volt city circuit. With 62.5 pulses a second turning the 10-pole motor, the rotor of the chronoscope made 6.25 revolutions a second when in operation; and, as the hand when rotating made one revolution for every five of the armature, it made 1.25 revolutions a second. The dial was divided into 100 equal units, so that the hand covered 125 dial units a second, making one dial unit equal to eight sigma. A cardboard screen was placed so as

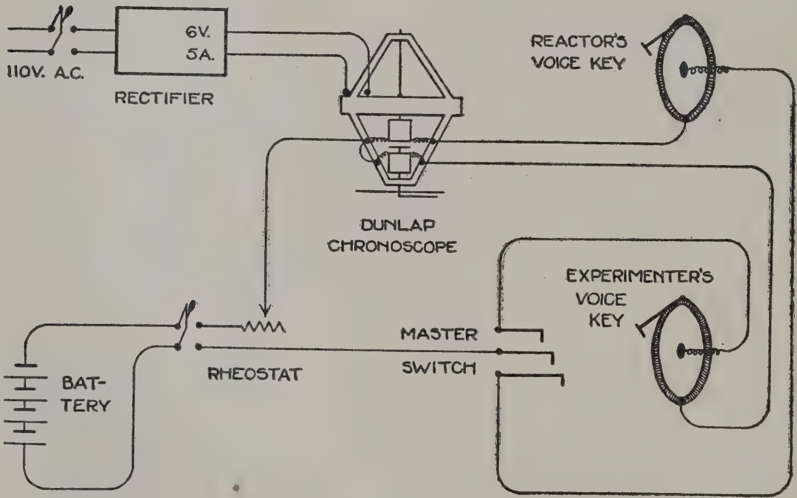

FIGURE 1
Connections of Dunlap Chronoscope and Voice Keys with Half-Wave Rectifier and Battery for Word Association Timing

to conceal the chronoscope, stimulus words, experimenter's records, etc., from the reactor.

Eight reactors, four men and four women, were used in this experiment. They were all graduate students in the Department of Psychology and all but one (G) had operated the apparatus and reacted in work on controlled association, using nouns and adjectives, in the class in experimental methods. G was given a few practice periods with nouns and adjectives, and all the subjects adjusted themselves quickly and efficiently to the work of reacting. Reactors B, C, F, and H were men, and A, D, E, and G were women. They came individually at as nearly the same hour of the day as was possible, and for about four or three days a week until the 20 experimental days were completed.

The stimulus words are given in the following lists, which were repeated daily in each experiment, and which are identical with Finesinger's lists:

Experiment I. Adverbs: ably, blindly, bluntly, boldly, bravely, briefly, brightly, coldly, fairly, feebly, foully, freely, gently, gladly, glibly, harshly, jointly, lightly, loudly, partly, plainly, quickly, rashly, sadly, safely, sharply, shortly, shyly, slowly, slyly, snugly, sorely, surely, sweetly, wrongly. *Verbs*: accept, acquire, advise, blossom, chuckle, correct, crackle, declare, defeat, define, delay, devour, enter, fiddle, giggle, glitter, gossip, launder, marvel, mutter, patter, prolong, prosper, record, reform, revenge, shiver, shudder, sputter, stagger, stammer, surprise, totter, vibrate, visit.

Experiment II. Adverbs: angrily, audibly, bitterly, completely, distinctly, drunkenly, eagerly, easily, entirely, exactly, faithfully, finally, foolishly, gracefully, gratefully, greedily, handsomely, happily, heavily, honestly, horribly, lovingly, merrily, nervously, outwardly, painfully, peacefully, playfully, prettily, readily, sensibly, smilingly, terribly, worthily, wretchedly. *Verbs*: banish, combine, compute, convince, defend, degrade, depart, describe, dwindle, enjoy, enrage, erase, escape, exclaim, flatten, focus, flutter, hasten, insert, inspire, jingle, lament, linger, perceive, prevail, proclaim, quiver, rebuke, recur, remain, respond, select, struggle, subdue, vanish.

At the beginning of the experiment the reactor was seated at his voice key and, when adverbs were to be given first, given the following instructions: "You will be given a number of adverbs as stimulus words. Please give responses consisting of verbs which might be modified by the adverbs given. Give the first verb which the adverb suggests and which it could qualify." The experimenter gave a ready signal approximately one second before each stimulus word, pressed the master key so that current would be flowing in both magnets of the chronoscope, then spoke the stimulus word into his voice key. The hand thus set in rotation was stopped by the reactor speaking the response word into his voice key and showed the time taken for the response. The experimenter then recorded the response word and the number of dial units, set the hand back at zero, and proceeded to the next stimulus word. No stimulus word was repeated, even though the reactor may have failed to understand it.

At the end of the first list of 35 words the reactor was given a rest period of about five minutes, which was usually spent in irrelevant conversation with the experimenter. At the conclusion of the rest period a second list of 35 words was given; when these were verbs the instructions were to give the first adverb the verb suggested and which could qualify it. The procedure otherwise was the same as before. On the next experimental day the same words were given, but if the adverb list was given first on one day it was given last the next day, first the day after, and so on. Half the reactors had the adverb list first on the first experimental day and the other half started out with the verb stimulus list first. The order of the words within the lists was shuffled from day to day. Each experiment comprised ten consecutive experimental days.

A total of about 10,707 association reaction times were taken, 5270 from the first experiment and 5437 from the second, the maximum for each being 5600. The remaining cases were those in which (*a*) the reactor failed to respond, (*b*) the reactor failed to speak forcibly enough into the voice key to stop the chronoscope or the experimenter failed to start it, (*c*) the stopping of the chronoscope was obviously not due to the response word. The instrumental errors were troublesome only in the first part of the first experiment; they were reduced to a minimum by frequent cleaning of the voice key contacts and chronoscope clutch, by adjustment of the rheostat, and frequent charging of the Edison battery.

Some time after the conclusion of the chronoscope work lists were made up for each reactor of his most frequent response word to each stimulus word. In case several response words to one stimulus word had the same maximum frequency they were all placed in the list. These lists were presented to the reactors with the instructions to make simple sentences from the word pairs in the list. Finesinger's

(5) technique was altered in two respects. Instead of presenting all the word pairs with the stimulus word in each first at one time and the response word first at a repetition of the list a week later, the place of the stimulus word in the pairs was alternated from first to last in a haphazard order obtained by flipping a coin. As it was thought that the sentences made verbally would be more comparable to the verbal association reaction than would written sentences, the list was placed inside a paper folder with a slit, and as the reactor pushed the word pairs past the slit he gave verbally a sentence for each pair. The experimenter recorded, unseen by the reactor, whether the verb or adverb came first in the sentence and for part of the cases whether a subject, object, prepositional phrase, or other adverb came between the verb and adverb given. Each reactor gave sentences for two lists of 35 or more word pairs for each experiment. After the first presentation of each list it was cut down the middle, pasted together so that the words in the pairs were in reverse order, and presented again to the reactor for sentences about a week later. It was hoped that in this way some indication could be gotten of the relative frequency of adverb-verb and verb-adverb word order in each reactor's verbal behavior.

For statistical analysis of the data there were obtained for each control for each day for each reactor the distributions by 12.5 dial units (100 sigma) and the means and mean variations of the association reaction times. In computing these measures the association times were included of all responses which were listed as adverbs or verbs respectively in Webster's *New International Dictionary* and of all others which clearly had a verbal or adverbial meaning. The association times of responses such as the following were included: prepositional phrases with adverbial meaning ("in the springtime"), and manufactured words ("gloyfully," "smitch") which the reactor reported as having thought of as adverbs or verbs respectively. These constituted a very small proportion of the total number of responses. Participles were accepted as verb responses, but the reactor was asked to try to confine himself to the non-participial forms of the verbs, inasmuch as participles are often used with an adjective meaning. Tautological responses such as "stammeringly" to "stammer" were treated in the same way.

The means and mean variations for each day are given in Tables 11 to 26 and show in most cases a decrease from day to day, varying in amount and regularity from one reactor to another and also between controls and experiments for the same reactor. This general decrease in the means and mean variations indicates a general increase in rapidity and uniformity of the response times as the experiment progressed. The distributions have the usual form mentioned by many investigators of the association reaction.

Throughout the tables and the rest of the discussion the control in which an adverb stimulus word was given and a verb response called for is denoted as adverb-verb or *A-V*; the other control, in which a verb stimulus word was given and an adverb response called for is denoted as verb-adverb or *V-A*.

Table 1 shows the number of days in Experiment I in which the mean of the *A-V* association times is shorter than the mean of the *V-A* times. For reactors C, D, E, F, and H the average *A-V* time is shorter than the average *V-A* time more than half of the days, for reactor A it was shorter half of the days, for reactors B and G less than half. It will be noticed that for all the reactors but G the greater number of the shorter *A-V* average times came in the first five days. Of all days for all reactors, the *A-V* average time was shorter than the *V-A* average time 65% of the time.

TABLE 1

EXPERIMENT I

NUMBER OF DAYS IN WHICH AVERAGE OF *A-V* TIMES IS SMALLER THAN AVERAGE OF *V-A* TIMES

EXPERIMENT I

Reactor→	A	B	C	D	E	F	G	H	Total
Days 1 to 5	4	3	5	5	4	5	1	5	32
Days 6 to 10	1	0	3	3	2	3	3	5	20
Total	5	3	8	8	6	8	4	10	52

Percentage of total cases that *A-V* average time is shorter—65%.

TABLE 2

EXPERIMENT II

NUMBER OF DAYS IN WHICH AVERAGE OF *A-V* TIMES IS SMALLER THAN AVERAGE OF *V-A* TIMES

Reactor→	A	B	C	D	E	F	G	H	Total
Days 1 to 5	3	3	4	1	3	2	4	5	25
Days 6 to 10	2	3	4	2	3	2	2	1	19
Total	5	6	8	3	6	4	6	6	44

Percentage of total cases that *A-V* average time is shorter—55%.

TABLE 3

NUMBER AND PERCENTAGES OF REACTORS FOR WHOM AVERAGE *A-V* TIME WAS SHORTER, BY DAYS

Day→	1	2	3	4	5	6	7	8	9	10
Experiment I										
Number	7	6	7	6	6	6	4	2	3	5
Percentage	88	75	88	75	75	75	50	25	38	63
Experiment II										
Number	5	6	5	5	4	4	4	4	3	4
Percentage	63	75	63	63	50	50	50	50	37	50

Table 2 shows a similar tabulation for Experiment II. In this experiment the adverb stimulus words have one more syllable than the verb stimulus words. The relation between the *A-V* and *V-A* average times found in Experiment I still holds, but is less pronounced. Reactor A still has the average *A-V* time shorter on five days and longer on the other five, but a different two reactors, D and F, have the average *A-V* time shorter on less than half the days. The others, B, C, E, G, and H, have the average *A-V* time shorter more than half the time. The percentage of total days for all reactors in which the average *A-V* time is the shorter is still more than half (55%) but only slightly so. Table 3 shows from another angle the tendency mentioned above for the *A-V* average times to be shorter for a large proportion of the earlier days of the experiment, but to be more nearly equal in number to the shorter *V-A* times in the later days.

Tables 4 and 5 give for each reactor the means of all his *A-V* and *V-A* times for each experiment. It will be seen by reference to

TABLE 4

EXPERIMENT I

AVERAGES OF ASSOCIATION TIMES FOR ALL TEN DAYS IN SIGMA

Reactor	*A-V*	*V-A*	*A-V* shorter by	*V-A* shorter by
A	1130.32	1183.92	54	
B	1311.84	1307.04		5
C	1094.16	1163.68	70	
D	808.56	903.28	94	
E	1458.16	1620.32	162	
F	1157.12	1306.08	149	
G	1996.16	2015.20	19	
H	1468.96	1700.56	232	

TABLE 5
EXPERIMENT II
AVERAGES OF ASSOCIATION TIMES FOR ALL TEN DAYS IN SIGMA

Reactor	*A-V*	*V-A*	*A-V* shorter by	*V-A* shorter by
A	1376.24	1400.88	25	
B	1531.76	1653.84	122	
C	1314.56	1376.16	62	
D	943.68	910.08		34
E	1779.60	1860.88	81	
F	1380.08	1317.84		62
G	2001.92	2070.56	69	
H	1365.84	1407.04	41	

these tables that in Experiment I the average of all the *A-V* times is smaller than that of the *V-A* for all reactors but B, and is practically equal for him. In Experiment II all reactors but D and F have the average of the *A-V* times smaller than that of the *V-A*.

An interesting point is that for A, B, C, E, and G the average *V-A* time in Experiment II is substantially longer than in Experiment I. This is in spite of the fact that the stimulus verbs in both experiments were dissyllabic, and it would naturally be expected that in the second experiment these reactors would respond more quickly from practice. Since the *A-V* times were a good deal longer in the second experiment than in the first it may be that these reactors came to habitually maintain to some extent a certain set in speed of response during an experimental period.

On the other hand, H's responses for both controls are much quicker in the second experiment than in the first. He confessed to an attitude of searching for unusual and recondite responses at the beginning of the experimental work and to changing in the early days of the second experiment to an attitude of less solicitation about the character of the response word and of quicker response. This does not necessarily mean disobeyal of instructions.

Table 6 shows the percentage of response words repeated by reactors for each experiment. A response word was considered a repetition if it had been given one or more times before during the experiment to the same stimulus word. With three exceptions (C in Experiment I and C and F in Experiment II) there are slightly fewer response words repeated to adverb stimuli than to verb stimuli. These differences can hardly be considered large enough to account for the differences in reaction times between the two controls.

TABLE 6

PERCENTAGES OF REPETITIONS OF RESPONSES

Reactor→	A	B	C	D	E	F	G	H
Experiment I								
A-V	58	65	72	53	42	65	44	46
V-A	62	72	71	61	48	71	30	56
Experiment II								
A-V	56	61	70	42	31	68	38	55
V-A	62	66	63	44	39	62	43	57

TABLE 7

PERCENTAGES (WEIGHTED) OF SENTENCES IN WHICH REACTORS USED THE VERB-ADVERB WORD ORDER

	Experiment I				Experiment II			
	Adverb stimulus, verb response words		Verb stimulus, adverb response words		Adverb stimulus, verb response words		Verb stimulus, adverb response words	
Presentation→ Reactor	I	II	I	II	I	II	I	II
A	56	48	78	85	44	73	61	79
B	74	60	54	69	66	60	70	61
C	100	99	94	100	100	100	100	100
D	97	94	90	72	90	90	87	87
E	66	80	65	68	45	23	58	51
F	94	94	84	79	86	80	60	60
G	84	83	76	64	71	59	46	44
H	99	93	100	91	85	89	91	74

Table 7 gives the results from the sentences made up by the reactors from the stimulus words and their most frequent response words. The percentages are weighted in the following manner: If there were two equally frequent responses to a given stimulus word, then the word order for each was given half the value of that given to a word order for a single most frequent response word. The weighting, however, gave results little different from those obtained without weighting. This table shows that for all reactors the word order given in their sentences was almost always a verb-adverb word order in a majority of cases. Only 6 exceptions are found out of 64 cases: These are two each for reactors A, E, and G, and all but one of these range from 44% up. This indicates that in the reactors' conversation the predominant order is verb-adverb.

TABLE 8

SEQUENCE OF VERB AND ADVERB IN SENTENCES GIVEN BY REACTORS

Reactor and presentation		Experiment I						Experiment II					
		Adverb stimulus word			Verb stimulus word			Adverb stimulus word			Verb stimulus word		
		AV	*VA*	*VoA*	*AV*	*VA*	*VoA*	*AV*	*VA*	*VoA*	*AV*	*VA*	*VoA*
A	2	20	15	4	11	26	14	11	12	15	9	17	16
B	2	14	17	5	11	20	9	14	13	8	17	17	6
C	2	1	26	11	0	25	10	0	37	0	0	23	16
D	1							3	27	9	5	25	16
	2	2	20	15	11	21	8	4	31	5	6	25	17
E	1							31	15	8	24	14	11
	2	18	33	7	19	22	5	40	11	4	26	14	9
F	1							5	13	18	13	9	13
	2	2	19	20	8	16	12	6	15	14	14	10	12
G	1							14	21	18	23	12	9
	2	6	19	18	16	19	7	16	21	14	24	11	8
H	1							7	21	14	3	17	15
	2	4	30	20	3	25	9	6	22	14	9	17	9

AV=number of sentences in which adverb was given directly before verb.
VA=number of sentences in which verb was given directly before adverb.
VoA=number of sentences in which verb was given first, then intervening word or words, then adverb.
Number of cases in which *AV* is greater than *VA*=10
Number of cases in which *VA* is greater than *AV*=31=74%
Number of cases in which *AV* and *VA* are equal=1

TABLE 9

TIMES THAT SERIES GIVEN HAS SHORTER AVERAGE REACTION TIME

Reactor→	A	B	C	D	E	F	G	H	Total
Experiment I									
A-V first	4	0	4	4	4	5	4	5	30
V-A first	4	2	1	1	3	2	5	0	18
Experiment II									
A-V first	4	3	5	2	3	4	5	4	30
V-A first	4	2	2	4	2	5	4	3	26

Total for Experiment I =48; for Experiment II=56.
Percentage of all cases (80)
for Experiment I=60; for experiment II=70.

Table 8 shows the number of sentences for each reactor in which the adverb was given directly before the verb (*A-V*), in which the verb was given directly before the adverb (*V-A*), and in which the verb was given first, then an intervening word or words (object, infinitive, other adverb, prepositional phrase, or the like), then the

TABLE 10

NUMBER OF ASSOCIATION REACTION TIMES LONGER THAN 3000 SIGMA

Reactor	A	B	C	D	E	F	G	H
Experiment I								
A-V	1	9	1	0	17	1	15	9
V-A	0	9	0	1	18	11	23	20
Experiment II								
A-V	1	18	1	0	20	3	15	5
V-A	0	22	1	0	23	6	28	15

TABLE 11

EXPERIMENT I—REACTOR A

MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	139.03	32.04	167.03	41.45
2.	133.86	27.07	155.79	29.43
3.	135.94	30.05	171.31	37.28
4.	160.85	60.41	149.23	28.70
5.	134.10	30.79	146.58	34.63
6.	139.36	31.07	134.27	25.78
7.	136.59	28.30	135.09	28.12
8.	136.06	26.83	122.32	23.60
9.	144.03	35.04	157.65	42.58
10.	151.00	25.94	140.94	28.88
Av.	141.29		147.99	
Av. 1-5	140.76		157.99	

adverb (*VoA*). It will be seen from this table that, even disregarding the cases in which intervening words were used, the word order given in the sentences was predominantly verb-adverb.

Table 9 shows that in Experiment I 60% of the time, and in Experiment II 70% of the time, the list which was given first in the two-list period had the shorter average reaction time. This suggests an explanation for Browning's results with verb-adverb and adverb-verb controls, since he, for other reasons, presented the verb stimulus list always before the adverb stimulus list.

From a study of Table 10 it can be seen that the longer responses, specifically those over 3000 sigma, are neither predominantly adverb-verb nor verb-adverb except with G and H, and the greatest differ-

TABLE 12

EXPERIMENT I—REACTOR B

MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	193.49	57.32	292.24	157.88
2.	196.78	96.33	182.21	47.68
3.	151.63	33.00	160.81	38.96
4.	180.68	56.70	154.93	27.99
5.	128.09	32.06	134.94	27.51
6.	148.09	43.53	141.23	38.51
7.	155.14	41.50	152.14	42.27
8.	156.03	71.74	153.40	60.40
9.	155.55	74.39	123.34	34.08
10.	171.46	79.07	138.03	29.37
Av.	163.98		163.38	
Av. 1-5	170.13		185.03	

TABLE 13

EXPERIMENT I—REACTOR C

MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	170.47	29.82	193.56	50.77
2.	139.05	17.50	143.89	20.30
3.	126.12	21.47	166.53	36.41
4.	139.64	25.86	144.61	33.24
5.	128.29	26.42	151.59	35.88
6.	135.06	26.42	136.03	29.39
7.	118.40	18.70	136.09	34.78
8.	145.36	25.09	138.31	25.24
9.	151.74	35.58	134.20	23.86
10.	118.63	16.74	121.89	18.77
Av.	136.77		145.46	
Av. 1-5	140.71		160.04	

TABLE 14
Experiment I—Reactor D
Means and Mean Variations of Reaction Times in Dial Units

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	132.58	31.38	166.70	33.30
2.	113.73	18.70	130.44	21.29
3.	120.35	19.73	140.97	36.59
4.	102.65	15.17	116.80	23.97
5.	99.82	12.59	113.76	20.45
6	98.29	9.83	105.94	17.42
7.	86.14	12.85	98.43	16.30
8.	92.17	10.19	89.15	16.23
9.	83.66	10.90	83.11	10.58
10.	85.21	8.45	90.71	13.79
Av.	101.07		112.91	
Av. 1-5	113.83		133.73	

TABLE 15
Experiment I—Reactor E
Means and Mean Variations of Reaction Times in Dial Units

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	174.88	61.22	219.45	88.07
2.	188.21	90.54	241.00	100.86
3.	182.17	63.90	252.88	98.21
4.	153.77	37.65	207.59	79.18
5.	178.91	57.42	178.65	65.17
6.	190.46	89.97	207.13	79.13
7.	178.15	55.43	157.53	37.30
8.	207.20	75.10	185.06	63.84
9.	194.64	64.26	171.89	46.43
10.	170.00	45.63	185.53	51.78
Av.	182.27		202.54	
Av. 1-5	175.59		219.91	

TABLE 16

EXPERIMENT I—REACTOR F

MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

Day	Adverb-Verb Mean	Adverb-Verb Mean variation	Verb-Adverb Mean	Verb-Adverb Mean variation
1.	183.24	48.49	291.09	104.69
2.	177.77	44.54	199.67	60.62
3.	151.88	27.50	164.44	33.37
4.	144.09	31.17	154.60	31.12
5.	131.45	23.34	156.60	26.58
6.	146.77	29.54	156.03	38.27
7.	136.46	23.46	137.54	30.24
8.	133.91	30.88	117.37	19.55
9.	116.61	20.78	140.76	28.93
10.	121.74	22.91	116.00	20.00
Av.	144.64		163.26	
Av. 1-5	157.69		193.28	

TABLE 17

EXPERIMENT I—REACTOR G

MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

Day	Adverb-Verb Mean	Adverb-Verb Mean variation	Verb-Adverb Mean	Verb-Adverb Mean variation
1.	296.15	83.86	211.96	47.42
2.	307.88	90.54	296.14	89.32
3	291.56	57.20	288.39	59.77
4.	252.14	45.19	292.95	70.49
5.	257.52	32.02	243.29	23.68
6.	228.85	39.59	257.60	49.68
7.	234.06	42.41	210.38	47.30
8.	225.48	39.29	255.54	55.48
9.	218.68	34.61	209.31	35.09
10.	216.97	30.78	249.41	46.57
Av.	249.52		251.90	
Av. 1-5	281.05		266.55	

TABLE 18

Experiment I—Reactor H

Means and Mean Variations of Reaction Times in Dial Units

Day	Adverb-Verb Mean	Adverb-Verb Mean variation	Verb-Adverb Mean	Verb-Adverb Mean variation
1.	176.55	41.61	229.38	72.56
2.	206.27	34.55	293.11	119.93
3.	214.24	65.89	272.66	102.88
4.	228.00	86.79	243.76	83.01
5.	195.94	54.51	222.18	65.77
6.	199.29	80.21	231.90	73.06
7.	169.23	59.81	177.00	50.80
8.	131.47	27.24	135.67	45.09
9.	172.35	67.36	195.00	61.94
10.	141.09	35.17	143.16	35.85
Av.	183.62		212.57	
Av. 1-5	204.20		252.22	

TABLE 19

Experiment II—Reactor A

Means and Mean Variations of Reaction Times in Dial Units

Day	Adverb-Verb Mean	Adverb-Verb Mean variation	Verb-Adverb Mean	Verb-Adverb Mean variation
1.	206.16	33.16	203.85	35.82
2.	172.39	36.14	205.79	35.40
3.	172.91	36.33	176.71	29.02
4.	176.36	29.61	194.29	44.15
5.	185.03	34.11	179.35	28.36
6.	164.66	28.97	166.85	29.13
7.	173.44	23.91	160.33	26.95
8.	164.50	24.74	153.50	18.63
9.	156.35	18.26	155.82	24.89
10.	152.00	24.76	155.94	25.88
Av.	172.03		175.11	
Av. 1-5	182.57		192.00	

TABLE 20
EXPERIMENT II—REACTOR B
MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	277.91	109.10	294.52	130.70
2.	251.13	66.82	312.55	157.42
3.	221.94	66.99	211.68	67.44
4.	222.31	65.52	220.86	82.72
5.	159.69	26.06	210.09	67.85
6.	163.94	30.67	181.70	48.68
7.	144.37	21.65	182.83	56.99
8.	158.62	41.92	154.89	38.65
9.	160.91	48.36	141.49	30.87
10.	164.03	54.16	176.83	64.60
Av.	191.47		206.73	
Av. 1-5	226.60		249.94	

TABLE 21
EXPERIMENT II—REACTOR C
MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	190.58	36.84	185.48	53.89
2.	162.74	30.51	189.86	36.86
3.	165.64	27.76	191.29	46.09
4.	160.29	30.17	177.53	41.03
5.	167.89	26.55	168.31	34.88
6.	155.71	27.09	171.91	36.51
7.	152.35	27.28	154.60	28.14
8.	155.46	30.98	161.66	33.43
9.	167.97	33.74	153.89	32.27
10.	165.50	38.85	167.11	38.25
Av.	164.32		172.02	
Av. 1-5	169.43		182.49	

TABLE 22

Experiment II—Reactor D

Means and Mean Variations of Reaction Times in Dial Units

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	119.03	14.22	131.40	19.66
2.	131.42	17.91	128.29	20.86
3.	131.43	16.08	120.54	12.79
4.	126.15	16.70	109.89	12.97
5.	120.91	13.92	117.26	12.73
6.	123.11	14.37	114.46	15.03
7.	116.14	15.83	103.95	16.22
8.	103.94	14.05	105.26	14.85
9.	103.44	14.10	105.35	12.00
10.	103.46	10.50	100.69	10.07
Av.	117.96		113.76	
Av. 1-5	125.78		121.48	

TABLE 23

Experiment II—Reactor E

Means and Mean Variations of Reaction Times in Dial Units

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	231.80	54.22	241.91	83.20
2.	267.11	55.90	236.74	70.34
3.	225.69	66.67	269.56	88.84
4.	201.59	58.18	229.71	66.08
5.	234.17	66.28	220.00	67.49
6.	192.94	55.30	275.46	99.32
7.	190.09	46.77	217.94	57.48
8.	189.35	32.77	211.40	53.20
9.	236.94	66.91	198.14	46.29
10.	252.37	80.58	227.64	60.33
Av.	222.45		232.61	
Av. 1-5	232.07		239.58	

TABLE 24
EXPERIMENT II—REACTOR F
MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	215.82	59.61	205.17	64.24
2.	171.24	40.68	207.17	64.08
3.	185.34	38.47	172.35	42.75
4.	171.24	49.23	177.23	57.35
5.	194.26	47.15	162.79	43.63
6.	185.23	51.89	171.06	51.12
7.	183.03	53.57	139.80	23.52
8.	145.11	28.82	152.23	29.92
9.	152.74	28.81	128.91	24.71
10.	124.32	17.25	135.88	22.86
Av.	172.51		164.73	
Av. 1-5	187.58		184.94	

TABLE 25
EXPERIMENT II—REACTOR G
MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	251.14	40.76	289.64	78.97
2.	258.58	39.41	262.32	62.13
3.	231.63	35.52	262.27	48.32
4.	260.91	39.54	253.51	41.45
5.	239.68	46.58	286.91	63.81
6.	253.09	44.96	234.03	37.21
7.	257.63	60.99	263.66	59.11
8.	269.17	55.56	234.54	43.30
9.	224.09	52.51	252.49	46.64
10.	259.77	49.90	249.88	53.73
Av.	250.24		258.82	
Av. 1-5	248.39		270.89	

TABLE 26

EXPERIMENT II—REACTOR H

MEANS AND MEAN VARIATIONS OF REACTION TIMES IN DIAL UNITS

	Adverb-Verb		Verb-Adverb	
Day	Mean	Mean variation	Mean	Mean variation
1.	233.31	75.31	289.73	120.96
2.	205.83	44.16	206.75	62.47
3.	196.03	62.97	196.82	44.17
4.	165.42	31.98	173.26	43.05
5.	169.41	43.16	190.88	48.86
6.	158.40	29.91	152.91	37.19
7.	152.09	33.64	140.91	27.09
8.	146.59	25.25	129.03	20.17
9.	134.43	26.25	140.03	33.63
10.	146.84	36.97	139.00	31.41
Av.	170.73		175.88	
Av. 1-5	194.00		211.49	

ence (13, between 15 and 28 for G, Experiment II) amounts to less than 2% of the total number of this reactor's responses for Experiment II (700).

In Tables 11 to 26 there are given, besides the means for each day and for the whole series, the averages of the means for the first five days of each series. It will be noted that for all the reactors (except E and F in Experiment II) the differences between the averages of the *V-A* and *A-V* means for the first five days are much greater than those between the averages for all ten days. This corroborates the finding suggested in Tables 1 to 3 that increasing automatization of response tended as the experiment progressed to reduce the difference in speed between the two controls.

CONCLUSIONS

1. In Experiment I, wherein 8 reactors gave adverb responses to 35 dissyllabic verbs and verb responses to 35 dissyllabic adverbs each day for ten days, the adverb-verb reaction is found to be quicker in 65% of the cases.

2. In Experiment II, wherein 8 reactors gave adverb responses to 35 dissyllabic verbs and verb responses to 35 trisyllabic adverbs each day for ten days, the adverb-verb reaction is found to be quicker in a slight majority of cases (55%).

3. The averages of the adverb-verb times are smaller than the averages of the verb-adverb times more often in the earlier days of the experiments than in the later.

4. No important differences were found between the number of repeated verb responses to adverb stimuli and the number of repeated adverb responses to verb stimuli.

5. The usual word order of the reactors as indicated by their sentences is verb-adverb, whether or not intervening words are considered.

6. The averages of association reaction times from the stimulus list given first are smaller from 60 to 70% of the time than those from the stimulus list given second.

SUMMARY

From the usual English word order it would be expected that the average adverb-verb controlled association reaction time would be longer than the average verb-adverb controlled association reaction time. The results of the two experiments reported here indicate the contrary, as do the results of Finesinger. The results from Experiment II are the more striking: While the adverb stimulus words have as many syllables of differential meaning as the verb stimulus words and an extra syllable besides, yet the adverb-verb reaction time is still the shorter in a majority of cases. Other factors must be sought which would outweigh the facilitation of the verb-adverb response coming from the predominating use of verb-adverb order in conversation.

One of these other factors may be found in the possibility that we may use verbs in conversation much more frequently than adverbs. If verbs are more on the tip of the reactor's tongue from their more constant use and if there were more of them in his vocabulary, he would naturally be expected to come out with them more quickly. In order to find out how much of a factor this is, it would be necessary to follow the reactor around for a day or so into as many of his habitual activities and contacts as possible, record all his conversation, and analyze it for frequency and sequence of verbs and adverbs. However, a perusal of the response words gives the distinct impression that the adverbs given as response words are in fully as frequent use as the verbs given as response words. It would be impossible to say whether or not the adverbs given as response words are used with verbs in ordinary conversation as frequently as the verbs given as response words are used with adverbs. A second consideration is that

verbs and adverbs are not used exclusively with each other. It is possible that verbs are used without adverbs more often than adverbs are used with other parts of speech than verbs. Adverbs are used with adjectives and other adverbs as well as with verbs; the relative frequency of each use in actual conversation is more or less conjectural, and evidence from the prose of books, newspapers, and prepared speeches would be indicative but not as final as that from actual conversation. A third consideration, of course, is that with more reactors, different stimulus words, and more days in which stimulus words were repeated, the present findings would be reversed. But as matters stand now, with results from 16 reactors (including those of Finesinger), an explanation for the greater average speed of the adverb-verb response must be sought in factors other than the customary English word order.

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UNE COMPARAISON EXPÉRIMENTALE DES TEMPS DE RÉACTION D'ASSOCIATION CONTRÔLÉE POUR ADVERBE-VERBE ET VERBE-ADVERBE

(Résumé)

Le but de cette étude, c'est de trouver laquelle des réactions d'associations contrôlées est la plus rapide: celle où l'on donne des réponses adverbes aux stimuli verbes, ou celle où l'on donne des réponses verbes à des stimuli adverbes. On pourrait croire que celle-là serait la plus rapide, puisqu'en anglais le verbe est suivi ordinairement de l'adverbe. On a fait deux

expériences, huit étudiants universitaires étant les sujets qui ont réagi, et on a noté les temps de réponses au moyen des "voice keys" de Dunlap et d'un chronoscope Hopkins. Dans la première expérience il y a eu deux listes de stimuli composées respectivement de trente-cinq adverbes et verbes dissyllabes, et on a donné celles-ci à chaque sujet pendant dix jours. Puisque tous les adverbes ont terminé en "-ly," on a fait une autre expérience en employant de nouvelles listes de stimuli; l'une de verbes dissyllabes, l'autre d'adverbes trisyllabes. On a noté environ 10.707 temps de réponse. Dans la première expérience la réaction adverbe-verbe a été la plus rapide en 65 pour cent des cas et dans la seconde elle a été la plus rapide en 55 pour cent des cas. La tendance pour la réaction adverbe-verbe à être la plus rapide et a été remarquée plus fréquemment dans les premiers jours de l'expérience que dans les jours suivants où les réponses ont tendu à être plus automatiques. Les phrases que les sujets ont faites en employant chaque mot-stimulus et le mot avec lequel ils y avaient répondu le plus fréquemment ont montré l'ordre prédominant des mots verbe-adverbe. Aussi l'ordre ordinaire des mots en anglais n'explique-t-il pas les vitesses relatives des réactions d'associations contrôlées adverbe-verbe et verbe-adverbe. On discute d'autres facteurs.

Petran

EINE EXPERIMENTELLE VERGLEICHUNG DER ADVERB-VERBIALEN UND VERB-ADVERBIALEN ASSOZIATIVEN REAKTIONSZEIT

(Referat)

Der Zweck dieser Untersuchung war, zu erforschen, welche Art der kontrollierten Assoziation die schnellere sei,—die Art in der auf Verbreize mit Adverben oder die in der auf Adverbreize mit Verben reagiert wurde. Man könnte vielleicht erwarten, dass die erstgennante Assoziationsart die raschere sein würde, da auf Englisch das Adverb gewöhnlich dem Verbum folgt. Es wurden zwei Experimente ausgeführt an 8 Graduirten als Versuchspersonen, und die Reaktionszeiten (reaction times) wurden registriert mittels Dunlap'scher Stimmschlüsseln (Dunlap voice keys) und eines Hopkins-schen Chronoskops (Hopkins chronoscope). Im ersten Versuch gab es zwei Listen von Reizwörtern, bestehend aus je 35 zweisilbigen Adverben oder Verben und diese Listen wurden jeder Vp. während 10 Tagen präsentiert. Da die letzte Silbe jedes Adverbums aus "-ly" bestand, wurde ein weiterer Versuch ausgeführt in dem neue Listen von Reizwörten gebraucht wurden,—eine Liste bestehend aus zweisilbigen Verben und eine aus dreisilbigen Adverben. Es wurden im Ganzen ungefähr 10,707 Reaktionszeiten registriert.

Im ersten Versuch war die Adverb-Verb Reaktion in 65% der Fälle und im zweiten Versuch in 55% der Fälle die raschere. Die Tendenz der Adverb-Verb Reaktion die raschere zu sein war in den früheren Tagen des Versuchs leichter wahrnehmbar, als in den späteren, da später die Reaktionen im Allgemeinen automatischer wurden. In Sätzen, die von den Versuchspersonen selber erdacht wurden, in denen sie jedes Reizwort gebrauchten im Zusammenhang mit dem Worte mit dem sie am häufigsten darauf erwidert hatten, war die Wortfolge überwiegend Verb-Adverb. Die gewöhnliche Englische Wortfolge, also, erklärt nicht die relativen Schnelligkeiten der kontrollierten Adverb-Verb und Verb-Adverb Assoziationsreaktionen. Es werden andere Einwirkungen besprochen.

Petran

A STUDY OF THE EXPRESSION OF BODILY POSTURE*[1]

From the Psychological Laboratories of Cornell University

WILLIAM T. JAMES

It is curious that, although both the plastic and dramatic arts have long employed bodily posture as a means of expression, no serious study of the correlation between a posture and its expression has yet been made. The study of facial expression as a means of reading character was an ancient subject when Aristotle wrote his treatise on *Physiognomy*.[2] In modern times numerous investigations have also been made of the face as a medium of expression of emotion and thought. Gesture, too, because of its use in forensic art and as a substitute for spoken language, has been the subject of many researches. Bodily posture, however, has, in comparison with these, been neglected. Neither facial expression nor gesture can, it is true, be entirely dissociated from the body as a whole, and consequently in investigations concerning them references to posture are not infrequent. There are also occasional books on pantomime. There is again Darwin's (4) well-known treatise in which the expression of bodily postures is frequently discussed. There are, furthermore, books which, like Marey's (13), seek the expression of a continuous movement in a single phase of the movement. But the references are casual, the study of pantomime has centered upon the conventions of the stage; Darwin was searching for a biological theory, and Marey was concerned only with a single aspect of the total problem of bodily expression.

The studies of the field at large fall into two groups: those which seek to express a feeling by means of posture or movement, and those which try to find a single phase of movement which will express a total movement. The former group divides further, according as the expression is that of face, or of the hands and arms

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[1] This study was done under the direction of Professor H. P. Weld at Cornell University.

[2] (1, pp. 805ff.) This tract, a mixture of traditional and casual observation, is, of course, of little scientific value.

(gesture), or of the relative positions of head, trunk, arms, and legs (posture). These studies are, furthermore, made from different points of view which have involved different modes of procedure. Those which approach the subject from the biological point of view in general start with the posture or movement and then search for the emotion thereby expressed. Consequently, they are primarily interested in what is called the innate mode of expression. Those, on the other hand, that begin with the dramatic, the artistic, and at times the psychological point of view, start with the logically defined instinct or emotion and then seek a posture or movement that will fit it. Of the two modes of approach the former is undoubtedly the superior; it is safer with our present psychological knowledge to start with the expression which is empirically observed and proceed from it to the emotion or attitude expressed. This method has been limited, however, to those observable expressions which the investigator happens upon, and it has not been applied to the field systematically. A single facial expression or bodily posture may be regarded as consisting of a number of variable factors such that a change in any one might be expected to change the expression of the whole. A systematic study of the problem would therefore seem to require a method that will take into account all of the variables involved and investigate all constellations of the variables with as few logical or psychological assumptions as possible. This method has, in fact, been employed to a certain extent by a number of investigators of facial expression, but they have frequently failed to justify by an empirical method the emotion or attitude which they ascribed to a mimetic pattern. The method has not been employed with gesture and bodily posture, and it is with these constellations that we shall be concerned in this study. There is, of course, a certain artificiality in assuming the independence of mimicry, gesture, and posture as modes of expression. The natural expression, we may suppose, is a total made up of a certain facial expression, certain gestures, and a bodily posture. There is, however, no guarantee that expression may not be based upon some single aspect of the total. Furthermore, admitting the normal unitariness of the bodily expression, the separation of its components for study is justified, since in no other way can the relations of the parts in the total expression be understood.

Problem and Method of Attack

The purpose of this study is threefold: (1) to determine in how far bodily posture (apart from mimicry and gesture taken in the technical sense) may be expressive, and (2), if it is expressive, to determine the relative expressive values of the various parts of the total posture, and (3) to determine whether or not there is in the experience of the observer a correlated attitudinal or emotional pattern.

A first experiment was directed upon the first two of these parts, and subsequent experiments were concerned with the third part of the general problem. In Experiment I we presented to our observers a large number of postures and asked them to report what, if anything, the postures expressed, and to indicate the part of the total posture most significant for the expression. In Experiment II postures were presented to the observers who reported not only the expressive value of the posture, but also a description and characterization of their own experiential processes. In Experiment III the observers were asked to assume postures, and then to report the expression of the posture assumed. In the latter two experiments we hoped to get at the relation, if any, between the attitude or emotion as expressed by the posture and the attitude or emotion as felt by the observer.

MATERIALS

We employed in the study a total of 347 different postures. These were chosen with no prior assumption other than the expectation that various postures might be expressive. They were obtained by photographing a human mannikin on a 35-mm. film. The postures were then projected one at a time on a white screen in a dark room. The figure was dressed as simply as possible in white athletic trunks and shirt, and stood before a black background. Since we wished to study the expression of the posture without influence of facial expression, the mannikin wore a black mask.[3]

The five variables considered were head, trunk, feet, knees, and arms. The posture consisting of head erect, trunk erect, feet together, knees straight, and hands at the side was taken as basic. Each factor except the knees was first varied separately in every one of its possible ways, and then in combination with variations of the other factors. In Table 1 are listed the variable factors with the different

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[3] We acknowledge with gratitude the aid and interest of Mr. A. U. Moore, who acted as subject for obtaining the large number of postures employed in this study.

TABLE 1

Indicating the Different Positions of the Variables H (Head), T (Trunk), F (Feet), A (Arms), and K (Knees)

The basic posture: head erect, trunk erect, feet together, arms at the side, was varied as follows:

H—(1) head forward (2) head back (3) head turned right
T—(4) trunk forward (5) trunk back (6) trunk turned right
F—Taking into account distribution of weight; right foot forward (7) weight on forward foot (8) weight on both feet (9) weight on backward foot (10) right foot relaxed with weight on left; in this case the right foot was not advanced as in the other weight positions. Taking into account a wider separation of the feet (wide base, 20 inches)
A—(11) arms front, palms up (12) arms front, palms down (13) arms front, palms out (push off) (14) arms front, high, palms out (15) arms back at side (16) one arm front, palm up (17) one arm front, palm down (18) one arm front, palm out (push off) (19) one arm front, high palm out (20) one arm back at side
K—flexed; two crouching and one kneeling position

TABLE 2

The Organization of the Different Positions of the Variables in Groups and Series

GROUP I—Each part of the posture varied separately in every one of its possible positions, all other parts of the posture remaining as in the basic posture[4]
Series 1 H varied in positions (1, 2, 3)
Series 2 T varied in positions (4, 5, 6)
Series 3 F varied in positions (7, 8, 9, 10)
Series 4 A varied in positions (11, 12, 13, 14, 15, 16, 17, etc.)

GROUP II—Each part of the basic posture combined with every other factor, a total of ten series
Series 1 H forward, combined with 4, 5, 6, 7, 8, 9, 10, 11, 12, etc.
Series 2 H back, combined with 4, 5, 6, 7, 8, 9, 10, 11, 12, etc.
Series 3 H turned, combined with 4, 5, 6, 7, 8, 9, 10, 11, 12, etc.
Series 4 T forward, combined with 7, 8, 9, 10, 11, 12, 13, 14, 15, etc. (See Plate I, col. 1)
Series 5 T back, combined with 7, 8, 9, 10, 11, 12, 13, 14, 15, etc.
Series 6 T turned, combined with 7, 8, 9, 10, 11, 12, 13, 14, 15, etc.
Series 7 F—weight on forward foot (all positions of hands and arms)
Series 8 F—weight on both feet (all positions of hands and arms)
Series 9 F—weight on back foot (all positions of hands and arms)
Series 10 F—right relaxed, weight on left (all positions of hands and arms)

GROUP III—Combinations of H and T, with F and A
Series 1 H forward, T forward, combined with 7, 8, 9, 10, 11, 12, 13, etc. (See Plate I, col. 2)
Series 2 H forward, T turned, combined with 7, 8, 9, 10, 11, 12, 13, etc.

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[4] A few postures in this group were dropped because they were so unnatural as to preclude photographing them.

GROUP IV—Combination of H, T, and F, with A, a total of 9 series taking into account distribution of weight combined with accentuated forward or backward bend, or a turn of the trunk right that is greater than either of the bends or turns used in the above series.

Series 1 H forward, weight on forward foot (all positions of hands and arms)
(See Plate I, col. 3)

Series 2 H forward, weight on both feet (all positions of hands and arms)

Series 3 T forward, weight on back foot (all positions of hands and arms)

Series 4 H back, weight on forward foot (all positions of hands and arms)
(See Plate II, col. 1)

Series 5 H back, weight on both feet (all positions of hands and arms)

Series 6 T back, weight on back foot (all positions of hands and arms)

Series 7 H turned right, weight on forward foot (all positions of hands and arms (See Plate I, col. 4)

Series 8 H turned right, weight on both feet (all positions of hands and arms)

Series 9 T turned right, weight on back foot (all positions of hands and arms)

Series 10 T turned right, weight on left foot, right relaxed (all positions of hands and arms)

GROUP V (Crouch)—Flexion either forward or backward of the body, combined with different flexed knees

Series 1 Crouch extremely low. H forward, T low (close to knees). Angle of right knee 65°, left knee 55°. Right foot and weight slightly forward. (Observer to the right of figure.) Combined with hands, 11, 12, 13, 14, 15, 16, and on the chest. (See Plate II, col. 2.)

Series 2 Crouch slight. H and T slightly forward. Knees flexed, angle 145°. Weight evenly divided on two feet comfortably apart. Hands employed, 11, 12, 13, 14, 15, 16, 17, 18, and one arms akimbo. (Posture faced the observer. See Plate II, col. 2.)

Series 3 Same flexion of knee as above, head and trunk leaned backward. Hands employed, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20.

Series 4 Same as Series 1, with weight of body on the toes.

GROUP VI (Wide Base) Postures emphasize an increased distance, right and left, between the feet, weight equally divided upon feet. Width of base 20 inches. (Figure faced the observer.) Total of 30 postures in this group.

Series 1 Head and trunk erect, legs straight. Hands and arms employed, 12, 13, 14, 15, 16, 17, 18, 19, 20.

Series 2 Same as above except knees slightly bent, and head forward to slight degree.

Series 3 Sames as Series 2, except head leaned backward.

GROUP VII (Kneeling) Kneeling posture combined with same hands and arms as above, with a few omissions.

positions of each, and in Table 2 the organization in groups and series of the different positions of the variables.

Experiment I

In Experiment I every posture was presented to each of three

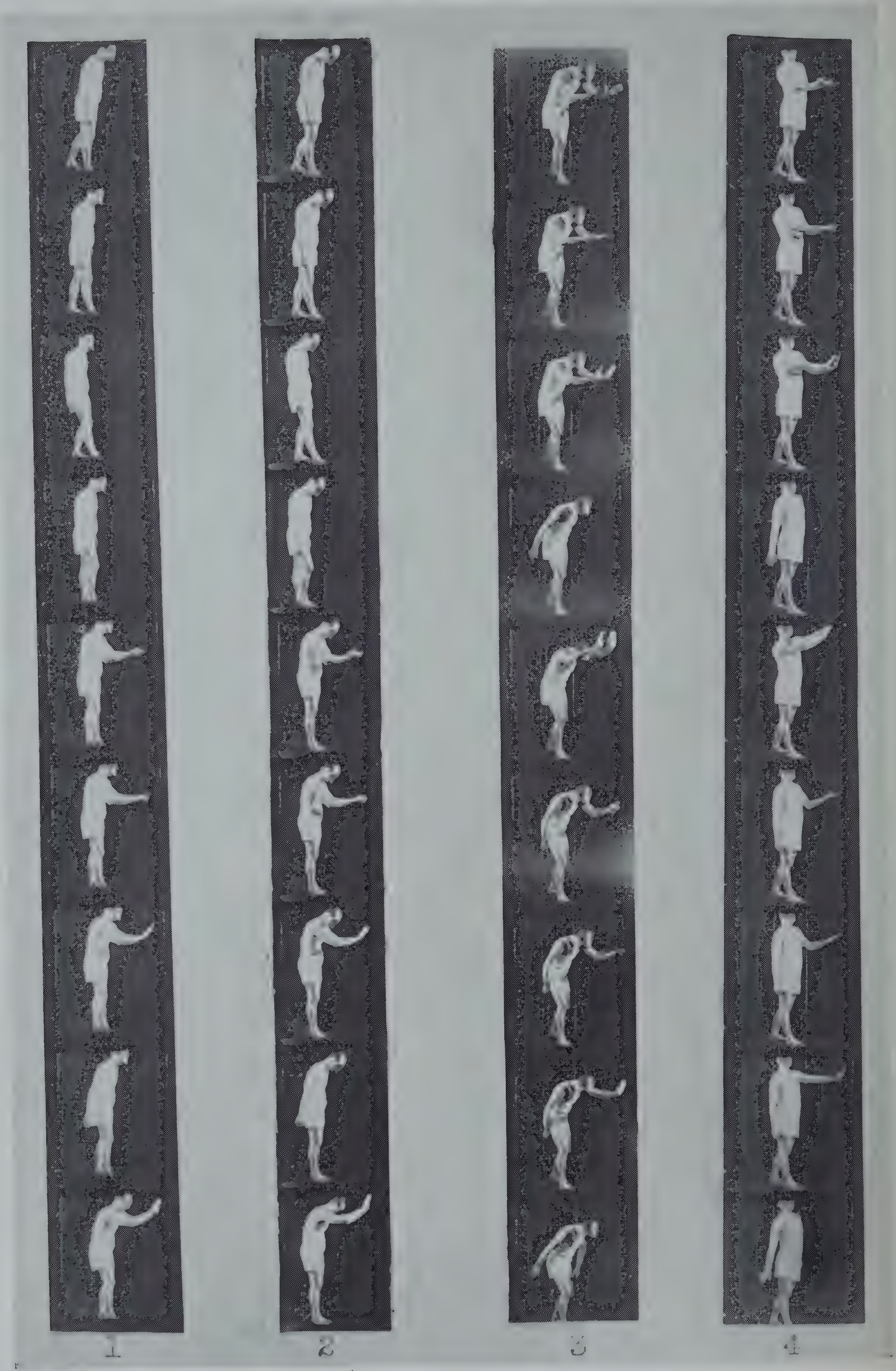

PLATE 1

1—Trunk forward series, Group II, Series 4.
2—Head forward, trunk forward, Group III, Series 1.
3—Head forward, trunk forward, weight on both feet. The more accentuated forward bend of Group IV, Series 2.
4—Head turned, trunk turned, weight on both feet. The accentuated turn of the head and trunk, Group IV, Series 8.

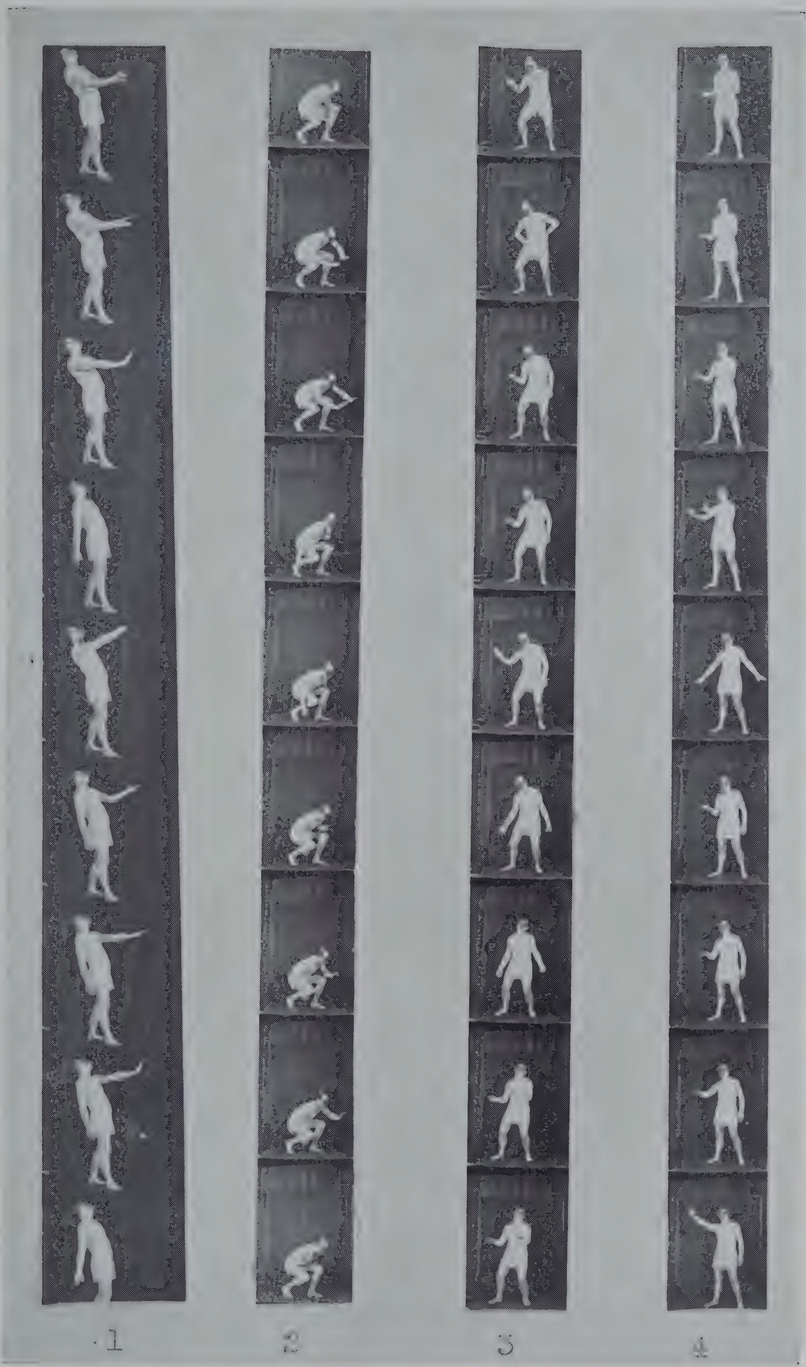

PLATE 2

1—Head back, trunk back, weight on back foot, Group IV, Series 6.
2—Crouch Series 1.
3—Six pictures of Crouch Series 2, and three pictures of Crouch Series 4.
4—Wide-base Series 1.

TABLE 3
THE GENERIC AND SPECIFIC EXPRESSION OF EVERY FACTOR OF THE POSTURE TAKEN SEPARATELY

Postural factor	Generic expressive value	Specifications
Forward (bowed) H	Withdrawal from the situation; the organism contracts within itself. *Contraction*	Thought, deliberation, embarrassment, shame, humility, weak sorrow, weak dejection, mediation
H turned	*a.* *Withdrawal,* turning away from object or situation *b.* *Approach,* turning toward object or situation	*a.* Shame, scorn, rejection *b.* Curiosity, interest, attention, alertness, slight surprise, poise, self-consciousness
H backward	*a.* *Expansion,* withdrawal in sense of 'ability to stand alone,' or 'desiring to stand alone' *b.* Reference based on line of regard, 'upward'	*a.* Disdain, *pride, haut,* denial *b.* The 'Seer,' star-gazer, prayer, benediction, viewing object above
T forward (H follows T slightly)	*a.* Approach, aggression *b.* Emotional predicament, inner condition — *contraction*	*a.* Attentive interest, intent scrutiny, curiosity, aggressive advance, amusement *b.* Defeat, rejection, dejection, chagrin, shame, sorrow, inferiority, tiredness
T backward	*a.* *Withdrawal,* in sense of moving away from situation *b.* General state of 'well-being' of organism, able to cope with situation, a mild withdrawal in sense of mastering situation, *expansion* *c.* Directional reference—line of regard	*a.* Negation *b.* Determination, courage, declaration, defiance, conceit, laughter, self-esteem *c.* Viewing object above posture
T turned (naturally turns H also)	Same as H turned	Same as H turned
Arms forward palms up (one or both)	*a.* Accepts *b.* Gives up *c.* Holds object *d.* Reference to object at distance (one arm)	(Specifications of the arms and hands occur as modifications of the expression of the combined factors of the total posture) (Table 4)

Arms forward, palms down (one or both)	*a*. Holds off object or idea *b*. Downward reference in movement or idea	*b*. Peace, 'be quiet,' blessing
Arms forward, palms outward, (one or both)	*a*. Facilitates withdrawal *b*. Holds off object, posture stands ground *c*. Movement forward	
Feet spread (wide base)	*a*. Firmness *b*. Strength of character *c*. Movement	*b*. Firm command, and with palms up, firm acceptance *c*. (with proper hands, about to catch ball)
Crouch I (Body near the ground)	*a*. Withdrawal *b*. Approach *c*. As movement	*a*. Primitive fear *b*. Primitive attack *c*. About to spring
Crouch II (Knees slightly bent)	*a*. General weakness, *Contraction* *b*. As movement	*a*. Exhaustion, helplessness, defeat *b*. A suddenly arrested motion
Kneeling	Withdrawal	Prayer, pleading

observers for an interval of 30 seconds with the following instructions:[5]

> "After the signal 'ready,' I shall show you a photograph of a bodily posture. Characterize briefly (1) what the posture signifies, i.e., what attitude is expressed, and (2) say, if you can, whether any part of the total posture is especially significant, and, if so, in what way."

Results. By far the greater number of reports show that the observers found in the various postures an expression of a mental state or function (thought or emotion). All observers occasionally, however, and one observer frequently, interpreted the posture as an expression of a kind of behavior, i.e., the figure was regarded as in process of movement. In presenting our results, therefore, we shall, in general, be dealing with the reports of the former kind; the latter, when they appear, will be indicated as such.

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[5] The observers who served throughout all of our experiments were O. D. Anderson (A), G. L. Freeman (F), and E. Moul (M). All were in the third year of graduate work at Cornell University. E. H. Kemp and A. D. Glanville, graduate students in psychology, also observed in a number of selected postures after the three formal experiments had been completed.

Our method, i.e., (1) the variation of every factor separately, and then (2) the systematic grouping of the factors in all possible combinations, has made it possible to determine the kind (or kinds) of expression which may attach to the several factors investigated. The terms employed by our observers when specifying the expression of any factor are related, as the same or synonomous, or similar in meaning. For example, the mental states symbolized by such terms as shame and embarrassment are either synonomous or perhaps represent a difference only in degree. The same may be said of 'dejection,' 'despair,' and 'defeat.' Terms, however, like humility, sorrow, and resignation represent mental states which in some ways are obviously different, although in other respects they are similar. All three groups of terms, however, may be said to belong to a more general category, all are species of 'mental depression,' of a 'withdrawal within the self,' and they may be regarded as specifications of the more generic meaning. This generic meaning frequently defies characterization by a single term, but it is usually at once recognized by the observer in his search for the particularizing term, which, when found, is often regarded as a particular instance of the more general state.[6] In listing the expressions of the postures, therefore, we shall give both types of meaning. A posture is, however, not always unequivocal as regards its generic meaning. One and the same factor may express different meanings and two different factors may have the same generic meaning.

In Table 3 we give the generic and specific expressions most frequently reported by the observers for the various postural factors. All of these expressions have been reported by every observer, and no expression has been accepted as characteristic of the posture unless it occurred a number of times in the report of separate observations. The generic expressive value is given in Column 2 of the table. When a posture had two or more generic meanings they are differentiated by the letters *a, b,* or *c.* In Column 3 we give the specific expressions reported when taken under the generic meaning *a, b,* or *c.*

Combinations of the Various Factors. Although we carried through the wide range of variations indicated in Table 2, it is neither feasible nor necessary to present in detail the mass of results obtained. The human body, although treated in our experiment as a mechanical mannikin, is, of course, that of an organism having not only a particular structure with particular muscular coordinations and innervations, but also one subject to the laws of gravitation. Consequently, certain postures required by our method were regarded by our observers as unnatural and as having no other expressive meaning. There are, moreover, other postures which are physiologically easy to assume but which require an unusual and, in this sense, unnatural muscular coordination. For example, if the trunk is carried forward or backward, the head tends to go with it; as a result, either the forward trunk with backward head, or the backward trunk with forward head, though easy to assume, is unnatural. Finally, there are postures which the human organism naturally, reflexly, assumes in various situations. In Table 3 it may be seen that the generic expressions of certain variables are the same or similar, whereas others are dissimilar. Our reports reveal that, in general, the variables having similar expressions go together, they are congruent; and those having dissimilar expressions belie or deny each other, they are incongruent. The various positions of the hands must, however, be regarded as indicative gestures, and as such they become congruent or incongruent with respect to the generic expression of the rest of the body. When the observers were confronted with a posture whose factors are incongruent, either they report the total posture as incongruent, unnatural, meaningless, or one or more congruent factors are taken as indicative of the expression of the total posture and the incongruent factor or factors are ignored. We may therefore simplify the presentation of our results concerning the combinations of the various factors by giving only those which are congruent. These we give in Table 4. The generic meanings of the total posture are in Column 1; the various positions of the hands which in part condition the specific meanings in Column 2; and the specific meanings themselves in Column 3.

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[6] We return to this problem of generic and specific expression below (pp. 431-434).

The above tabular presentation of our results needs comment. In the first place, it should not be forgotten that any single factor is, as we have shown in Table 3, expressive or, perhaps more accurately, has an expressive value; and although this single factor is always a part of a total posture it tends to maintain its generic meaning, whatever the generic meanings of the other factors of a particular posture may be. The forward head, for example, cannot be

TABLE 4

COMBINATION OF THE VARIOUS FACTORS

I. *Trunk, head, and arms forward, knees straight*[a]		
a. Approach	with palms up	acceptance, offering, coaxing, supplication, beseeching—all with humbleness
	with palms outward	active repulsion, avoidance, holding off, opposition, command, disapproval
	with palms down	soothing, calming, blessing; groping, balancing in movement; with slightly bent knees, reaching for support
b. Contraction	palms up	servitude, surrender (the figure offers itself)
	palms outward	dejection, grief, anguish, shame, defeat (with a refusal of aid or sympathy)
	palms down	(these hands add no meaning to the forward trunk or head)
II. *Trunk and head backward, arms forward, knees straight*[b]		
a. Withdrawal[c]	palms up	prayer, proud offer or acceptance
	palms outward	extreme negation, exaggerated refusal, repulsion, disgust; with bent knees (Crouch III), sudden recoil, horror, withdrawal from a dangerous position, arrogant refusal, pride
	palms down	blessing, proud dismissal, refusal
b. Expansion	palms up	joyful offering or receptiveness, welcome with great pride; with bent knees, prayer
	palms outward	
	palms down	(these hands add no meaning to the backward trunk or head when taken as an expansive posture)
III. *Trunk, head turned, arms forward*[d]		
	palms up	(hands in this position add no meaning to the turned trunk or head)

a. Withdrawal	palms forward	emphatic refusal, utter disgust or scorn or disdain, the object or observer is cast aside
	palms down	(these hands add no meaning to the turned head or trunk)

[a] The total posture is best when the weight of the body is thrown on the forward foot. Weight on the back foot is both unnatural and incongruent. The greater separation of the feet (wide base) has no effect on this posture.

[b] Arms backward is unnatural and when in that position are either ignored by the observer or else their meaning is the same as 'arms at the side.' The total posture is most effective when the weight is on the rear foot or is equally distributed on both feet with wide base. The latter position is at times said to add 'firmness' and 'strength' to the posture and its expressions.

[c] Bent knees change the withdrawal to a contraction which is further specified as weakness, helplessness.

[d] The feet and arms point in the direction of, and the trunk and head away from, the observer. The accentuated turn (Table 1, Group IV, Series 8) is difficult to assume and requires greater weight on the forward foot. The difference between the two degrees of 'turn' brought no difference in the reports on the two positions.

perceived independently of the other parts of the body, but it maintains its generic meaning in all combinations of other factors, although its specific meaning may vary in accordance with other factors.

In the second place, Table 4 does not reveal the fact that the various factors are not of equal weight for the determination of the total expression, nor do they contribute in the same way to the specific meaning put upon the total posture. In this respect the generic meaning of the position of the trunk is of most importance because reflexly it tends to determine the position of the head and the distribution of the weight of the body. The position of the head is in itself significant because it indicates the line of regard. The arms and especially the hands are, as has been said, indicatory gestures. The bent knees may, as a sign of weakness, change the generic meaning of a posture. Finally, our observers reported two sets of factors not included as such among our variables. The lowered chest and shoulders which reflexly accompanies the forward trunk and head, and the raised chest and shoulders which, in the same way, tend to follow the backward trunk and head were at times the significant factor for the determination of the specific expression of the posture, but we shall reserve their consideration until we have set forth the results of our other experiments.

EXPERIMENT II

The purpose of this experiment was to determine the nature of the experience of the observers during the presentation of the posture. We, in the first place, hoped to discover whether and, if so, under what conditions, the observer experiences the attitudes and emotions which he imputes to the observed posture, a question, it will be recalled, that other investigators of bodily expression have in general ignored. We desired also by appealing to the observer's experience to obtain direct evidence concerning the general conditions of postural expression and thus to supplement that obtained indirectly by inference from the expressions themselves.

In planning the experiment we chose as representative one posture, upon which there had been uniform agreement, from every group except the first of Table 2. These 30 postures were then presented one at a time to each of the three observers as in Experiment I. The observer was, however, asked to regard the posture as an expression of an emotion or attitude, and he was also asked to stand during the presentation. The reason for this latter variation was that casual remarks made by the observers in the earlier experiment led us to suspect a tendency on the part of the observer to imitate the posture presented. Since the posture was in the standing position we thought that the same position of the observer might facilitate a tendency to imitation if it existed. The instruction was as follows:

> "After the signal 'ready,' I shall show you a photograph of a bodily posture. Regard it as the expression of an emotion or attitude and report by way of comment and inspection the psychological processes which may occur."

Results. Since in this experiment the observer was instructed to regard the postural figure as an expression of an emotion or attitude, there are no reports of the posture as a phase of movement. The reports fall into groups according as the observer did or did not find any concomitant sensory, imaginal, or affective processes correlated with the verbal report of the expression. In the one of these two groups the expression was either named immediately as one might assign a name to any familiar object, or else the observer took an analytic attitude and based the expression upon parts of the total posture which were significant. In neither case was any concomitant experience reported. The relative frequency of these reports varied among observers as follows: for A, 13.3%; for M, 30.0%; and for F, 53%.

The remainder of the reports reveal, first, a tendency of the observer to imitate the posture presented. This tendency rarely resulted in an outright assumption of the posture, but the tendency was either directly reported or was revealed by reports of kinaesthesis in the arms, legs, back, neck, or other part of the body significant for the assumption of the posture. When the actual movement did not occur the observers frequently reported the experience as kinaesthetic imagery. Examples are:

> A: "A distinct tendency to approximate that position. The abdominal strain is most prominent." "A great deal of bodily stuff in experience, mostly strain and tightening of muscles in the arms and chest,—all of this coming after a very intense scrutiny of the figure. It meant a sort of supplication, a looking to higher powers."
>
> M: "I noticed the drag to the shoulders of the man before I put on any attitudinal or emotional meaning. I next noted a kinaesthetic feel, a similar droop in my own neck and shoulders. Then the picture became focal. The meaning I put on it was 'bodily weariness.' I don't think that my own bodily posture as regards hands and feet was a result of the posture presented. I think I was standing that way before; but the shoulders, head and neck very definitely followed my feeling of how the man in the picture felt." "I noted that the shoulders of the man were extremely far back, and that his abdomen was thrust forward. Before I got a meaning for the figure there were kinaesthetic strains in myself as if I were going to stand in the same way. I don't think I moved, however."
>
> F: "A tendency to accept the suggestion of the posture and assume it. This was carried out to the extent of lifting the chest and tensing the body. I felt alert, active and interested, and this meaning of the figure was that of an arrogant strut or swagger. To an extent I emulated the posture. The hips protruded slightly so that I got the feel for this sort of posture."

In some instances the observers also experienced the feeling or emotion which was attributed to the postural expression. The feeling was always reported as weak and ephemeral and resulted from the tendency of the observer to put himself into the same situation and posture as that of the figure. Again the relative number of reports of these experiences varied among the observers. A reported them 86.6%, M 70%, and F 46.6% of the observations. Typical reports follow:

> A: "My own attitude was almost overtly aggressive. There was a lot of bodily stuff present, abdominal pressures, that seem to rise just as I feel when I say 'You are a conceited ass.'" "My own attitude was to throw myself into the situation and momentarily I succeeded in doing this so well that I got the flash of pity." "The picture at once meant a sort of half-hearted, but dejected, hesitating plea. . . . I found that I also experienced a weak emotion of depression, a sort of uneasiness."
>
> M: "There was a slight emotional tone to the figure. The emotion was a sort of sympathy for something done in a dignified fashion by the figure. I felt as if I were a bit stirred up, grieved perhaps, in his begging. I felt the emotion he felt, but the dignity was in him."
>
> F: "The whole posture expressed buoyancy, joyfulness and abandon. . . . In a sense I felt the abandon myself." "Proud refusal. The meaning of pride seemed to be carried by the uplifted chin. I had a tendency to feel a sort of pride myself, at least a self-sufficiency. Refusal or rejection was expressed by the hands. There was, however, no tendency to put myself into that part of the posture."

In the above quotations from the reports both the kinaesthesis and emotions were localized in the observer's own person. In a few cases the observer projected himself empathically into the figure. The empathy seems to be conditioned in part at least upon the presence of the kinaesthesis or feeling.

> A: "The meaning is real as one is incipiently projected out there into the figure." "There was first a realization of the meaning of the figure, then a projection of self into the figure, then, an instant later, the reference to myself carrying the meaning of the figure."
>
> M: "The figure and myself seemed to be having the same experience. It is as if I read my experience into that of the figure and then I say 'the figure expresses determination.'" "Immediately I had a feeling of bending down, crouching in the figure presented, as if to coax a pet animal to come closer."

There can be no question that the fulfilment of the instruction to find an expression in the posture requires the observer to place the figure in a situation before the posture becomes meaningful. The relation between the expression and the situation in which the posture is placed will be discussed later (see below, p. 425). For the present it should be noted that when, as in the first group of reports discussed above, the figure is taken purely objectively the observer is not always aware that the expression which he assigns implies a situational context. In other cases the observer not only indicates the situation but he also reports reminiscences which are similar to and imaginal constructions which are characteristic of it. The following excerpts will serve as illustrations.

> A: "I had a clear idea of some little fellows with their school-books, then a memory of certain phases of my childhood." "The meaning that came of attending church-services and prayer being said by the minister." "I could feel myself kinaesthetically walking along the streets of Rome and the people calling me 'father.'" "I was posing in a gymnasium suit for a picture and boyishly wishing to exhibit my muscles. Momentarily I was an athlete."
>
> M: "There is no particular place-reference; the situation is general but like standing before a saint." "The figure was peaceful; the meaning was that of a man soothing a crowd before him, and as I looked at him I somehow identified myself as one of the crowd."
>
> F: "The meaning came at once as 'That is sorrowful' . . . There was a tendency to ideas of a sorrowful nature." "The meaning was 'an arrogant strut.' . . . I thought of the boastful business man with hands in pockets swaggering back and forth, and of a small boy doing the same thing."

There have been many indications that the tendency on the part of the observer to assume the posture was an aid in determining the expression of the posture. Our next experiment was designed to test the relation between judgments based upon the posture as seen and the posture as felt by the observer.

Experiment III

In this experiment we desired to investigate the relation between the postural expression as based upon the 'feel' of the posture as such and as upon the posture as seen. We did not, of course, presume that the kinaesthetic perception is purely kinaesthetic, and that the visual perception is purely visual. We have already seen that the visual perception is frequently accompanied by kinaesthesis, and it is probable that the kinaesthetic perception is, for some observers at least, accompanied by visual components. Nevertheless, we felt justified in making a comparison between the two modes of perception; for one thing, there is a difference in the relative focality of the variable factors in the two modes of experience, and, for another, there is no justification for assuming that the postural expressions found under the two conditions must be the same. Consequently we attempted the investigation. We employed the same 30 postures used in Experiment II with the following instruction:

> "After the signal 'ready,' I shall show you a photograph of a bodily posture. You are to assume the posture and hold it with closed eyes. Report the meaning of the posture assumed."

Results. The protocols show that, in general, the observers followed the instruction and reported the expression on the basis of the feeling of the assumed posture. Only occasionally, as, for example, when M said, "I spoiled this one; the meaning got attached to the figure before I had imitated it," was the expression based on the seen instead of the unseen felt posture. At times, however, there was nothing in the protocol itself to indicate the basis of the report. We give as illustrations three reports of the same posture—H and T erect, weight on both feet, one A up, palm out.

> F: "I was rather relaxed when I first assumed the posture, but as soon as I raised my hand I felt all the muscles of my body tensing, my shoulders settle, and I held my head up a bit. The meaning of all this was that I was alert and on guard, and perhaps a little angry. I did not notice any visceral disturbance."
>
> M: "I had the feeling that one has when he realizes that something unpleasant is about to happen, a kind of expectancy. It is the stretch of the right arm that does it. I am communicating my feeling to a crowd before me."
>
> A: "Means 'stop' as an exclamation. It refers to someone or something approaching."

The only way to bring the quantitative results of this experiment into relief is to compare them with those of Experiment I. The results obtained in Experiment II were obtained under a special instruction and are therefore incomparable. In about one-half of the total of 90 reports in Experiment III the expressions found were the same in generic meaning as those reported for the same postures in Experiment I. In half these cases (48.8%), about one-fourth of all reports, the terms employed were exactly the same; in the remaining cases of this group the meanings were so similar that we were unable to distinguish them. We were, for example, unable to find a difference in meaning between the following pairs of expressions: denial, negation; gesture of reference, explaining something; prayer, imploring; supplication, beseeching in prayer; bravado, boasting; disdainful and 'stay-off' attitude, refusal; pride (over-bearing attitude), arrogance; dejection with submission, an humble salaam or worshipful bow; grief, suffering. There are a few cases that are doubtful as, for instance, aggressive beckoning, and earnest pleading; passive negation, and passive gesticulation; laughter, and well-being or buoyancy; arrogant refusal, and disdainful.

Of the remaining cases, those in which the expressions found in the two experiments were obviously different, a few resulted from a difference in attitude. In Experiment I the observer reported a certain posture as the expression of an emotion or other mental attitude, in III he reported the same posture as the expression of a movement. In a few cases also (5.5% of the 90 observations) the posture assumed was felt to be so unnatural that it had no other meaning than 'unnatural,' whereas in Experiment I the unnaturalness was not noted and an expression of a mental attitude or a movement was reported. In other cases the difference resulted from a difference between the posture as seen and the posture as felt. For example, the posture head forward, trunk turned, weight on forward foot, arms forward, palms up in Experiment I for M meant beckoning; in Experiment III the same posture meant "a tug of war and I am on one end of the rope pulling." Again, head and trunk turned, right foot relaxed, arms at side in Experiment I for F meant surprise; in Experiment III it meant "something like pride—from the expansive feel of my height." About 10% of the 90 reports differed in this way from those on the same posture in Experiment I. It sometimes happens also that when the posture is assumed a muscular strain is more intensive in one part of the body than in another and that part becoming focal becomes most significant for the total posture and consequently gives it a meaning that was not found when the posture was visually perceived. This happened in 20% of the cases with M, in only 3% with F, and not at all for A. In a much larger number of cases (about 25% of the 90 observations) the difference was conditioned upon the different setting in which the observer placed the two postures. For example, in Experiment I, the posture head and trunk back, weight on both feet, arms forward and palms down meant for one observer 'pride and dramatic negation' whereas in Experiment III the same posture meant 'playful and comic negation.' In the former the figure was placed in a dramatic situation, in the latter the observer was playing with a child.

In sum, therefore, when we compare the results of this experiment with those of Experiment I, 50% of the reports were the same; 35% of those that were different resulted from a difference in situation, in attitude, or in respect to the part of the total posture that became focal; and about 15% of those that were different were based upon a difference between the posture as seen and as felt.

Discussion of Results

The Conditions of Postural Expression. We have given as briefly and as precisely as we could, the results of more than 1200 observations of 347 different postures, some of them under three different instructions. There are, of course, many other postures which we have not examined, but we believe that we have the materials for an adequate analysis of the problem of postural expression.

We may say at once that there are few postural expressions that are unequivocal to all men. For, from the point of view of the observer, the expression is in essence a meaning put upon a particular disposition of the various members of the body and, like other meanings, has numerous conditions.[7] The most important of these are the attitude of the observer, the situation in which the posture is placed, and the posture itself. These we shall discuss in order.

Attitude. We have seen (p. 405) that, historically regarded, the study of postural expression has fallen into two groups: the expression of a feeling or idea, and the expression of a movement. We have also seen (p. 413) that under the general instruction of Experiment I both forms of expression were found, and that under the specific instruction of Experiment II reports only of feeling or idea appear. Finally, we have noted that in Experiment I reports of movement were made much more frequently by one than by the other two observers. There would therefore seem to be no question but that which of the two forms of expression the observer reports will depend upon the predisposition of the observer. This attitude or disposition of the observer may be conditioned in several ways: (1) by instruction, (2) by habituation, e.g., we should expect a gymnast or a behaviorist to report movement, and an actor, emotion or idea; (3) by the situation or surroundings in which the posture appears, e.g., in a baseball game the erect figure with arms forward and palms up would almost certainly be labelled as a phase of movement, whereas on the rostrum it would as certainly be taken as an added expression of the spoken word; and (4) by the nature of the posture itself, e.g., the forward head and trunk is almost invariably taken as an expression of emotion and not as a phase of movement; on the other hand, the arms and hands stretched above the head tend to suggest a movement and not an emotion.

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[7] We return to a discussion of the nature of the expression below (p. 429).

There is still another way in which the disposition of the observer is effective for the expression given to the posture. Frequently it happens that some one part of the total posture is noted at the expense of the remaining part. That is to say, some one part catches the attention, and the expressive meaning put upon that part tends to color the expression of the total figure. The conditions which underlie this tendency are the same as those of primary attention. In particular, those parts whose expressive meaning is already known, those that are novel in the sense of the unusual, or the awkward, and those that evoke strain, discomfort, or other feeling in the observer himself when he either in whole or in part imitates the posture, catch the attention and in large measure condition the expressive meaning put upon the posture as a whole.

The Situation. We have already said that the surroundings in which the posture appears may condition the attitude of the observer. In everyday life, on the stage, and in pictorial art the posture always appears in objective surroundings natural to it or created for it. But in our experiments we attempted to eliminate the objective surroundings by dressing the figure in the simplest of clothing and photographing it against a neutral background. Although we were not entirely successful, for the figure was at times taken as an athlete or gymnast, most of our results show that our purpose was attained.

The elimination of an objective setting for the posture does not mean, however, that a posture may thereby be expressive without requiring or at least implying something else other than the posture itself. The observer must himself furnish the setting for the posture. In the reports quoted above from Experiment II it was seen that the observer is frequently not only aware of the setting which he furnishes, but his awareness is also accompanied by reminiscences or imaginal constructions which are similar to or constituent of the situation supplied. But even in those cases in which the expression is immediately reported and there is no awareness of the setting in which the figure is placed, a setting is nevertheless implied. For example, the generic meaning of the posture of Table 4, Posture 1, may be either an 'approach' or a 'contraction.' The difference in expression is conditioned upon a difference in the setting in which the posture is placed. In the one the setting must be objective, the figure faces some external object; in the other the setting must be subjective, the figure must be concerned with his own affairs. An objectively given setting may determine which of the two generic expressions will be given, but, lacking an objective situation, the result will depend upon the setting that the observer himself supplies.[8] And this in turn is, as we have said above, partly conditioned upon the observer's attitude. There is however one other important condition, and that is the posture itself. The situation consists not only of the setting but also of the figure which is in relation to the setting. Consequently, the figure must fit into the surroundings and the surroundings must be adapted to the figure. This means that the number of postures adaptable to a given situation is limited, and, conversely, the number of settings into which a given posture will fit is also limited. It follows that the posture itself determines in some measure its own setting. Every factor of the total posture has in this respect its own significance, but the position of the head with its 'directional reference' is perhaps of greatest importance; the object to which the posture is related will be found in the direction which the eyes take, and, since in this experiment the eyes were covered, the position of the head is the most important factor for suggesting the direction in which the object lies.

The Posture. The posture as a whole is an ensemble or constellation of different parts. Of these the head and trunk are, as we have seen (p. 417), the most significant for the generic expression of the total posture, and the hands and arms most important for the specification of the posture. The distribution of the weight of the body, the expansion or contraction of the chest, the raised or drooped shoulders are other factors which, each in its own setting, are important. Every one of these has in any particular position its own expression. But there may be a coincidence of these meanings with the result that the meaning of the total posture is in general the same as one of its parts. For example, the forward trunk, the forward head, weight on forward foot, the receptive hands, each and all mean approach to some external object or situation; the various factors are congruent. There may, however, be a difference in the relative importance of some of these meanings. The generic meaning of 'approach' is better expressed by the forward trunk, or outstretched hands, than by the forward (bowed) head. Any one, or all but one, of these parts may be reduced to zero and the meaning of the total posture preserved by the coincidence of the remaining factors, or by the significance of a single factor. The erect trunk, for example, means neither approach nor withdrawal, it is neutral or, let us say, zero as regards approach. The meaning of approach may still be maintained by the forward head with either weight on forward foot or receptive hands, or by receptive hands alone with head erect and weight on both feet. Finally, there may be in the total posture an opposition or difference in meaning of the various factors, i.e., one part may bear one meaning and another part a different meaning; the two factors are incongruent. For example, the backward trunk means withdrawal, the receptive hands mean approach. If the two are given together, with all other factors neutral, then it may be that only one of the two significant factors is noticed or becomes focal and gives its meaning to the total posture, or it may be that the observer himself notices the contradiction of the two factors and reports it. In some cases the two factors may supplement each other as, for example, when one factor means 'fear' and another factor means 'pleading,' and the two expressions combine as 'fear' and 'pleading,' there being no obvious reason why a frightened individual should not plead.[9]

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[8] The significance of the situation for facial expression has been remarked by several investigators. See C. Landis (9, p. 496), M. Sherman (16, p. 274), H. S. Langfeld (10, p. 179).

We have above (p. 415) referred to certain postures as natural and to others as unnatural; we have said that, in general, the natural posture is one that by virtue of nervous and muscular coordinations is easy to assume. Furthermore, it is indicated that if certain parts of the total posture were changed in position other parts would reflexly change their position in specific ways. For example, if the trunk moves forward from the neutral (erect) position as it rotates on the horizontal axis through the hips, the head tends to follow and, to maintain the equilibrium, one foot tends also to move forward. Conversely, if the trunk moves backward, the head naturally follows and one foot tends to move backward. The arms and hands seem in this respect to be independent of the remainder of the body. Consequently, we may say that reflexly regarded the position of the trunk is the most important factor for the total natural posture. This result is at variance with the neurological studies of animal posture, in which it has been found that in the spinal animal a change in the position of the head is accompanied by reflex adjustments of the position of the trunks and legs.[10] We do not, of course, pretend to say what the postural movements of man would be under the conditions of the animal experiments; but there can be no doubt that the assumption by man of the erect posture requires reflex adjustments for the maintenance of equilibrium that the quadruped, at least, does not possess. It might well happen, therefore, that in man the center of coordination might be in the trunk rather than, as in the dog, in the head. Furthermore, it may be remarked that since man is more proprioceptive than the lower animals we should expect not only a reflex pattern different from that of the lower animals, but also a greater independence of movement among the various parts of the total posture. We need not be surprised therefore to find that in man one factor may in a degree be changed in position without an appreciable change in that of the other factors.

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[9] These 'binary psychical compounds,' as Mantegazza (14, p. 167) calls them, were frequently reported in our experiment and they also appear in many other studies of expression. See Mantegazza (14, p. 167), Bell (3, p. 149). Some of the investigators of facial expression, particularly those that have made quantitative studies of the words employed to characterize various expressions, have separated the two terms and thus eliminated the double expression, a practice that does violence to the facts, however justified it may be statistically. [Feleky (6, p. 36), Frois-Wittmann (7, p. 120).]

Finally, something should be said concerning the relation of the posture as an expression of an idea or an emotion to the total movement of which the posture may be regarded as a phase. The human organism, of course, attains its posture by way of movement, and it might be argued that it is the movement as a whole that is expressive and, consequently, the posture taken by itself is an abstraction. Such an argument, however, would confuse the physiology of movement with the essential problem of expression. While it may be true that in some cases the total movement would be less equivocal than any of its phases, a single phase is sufficient for expression. Consider, for example, the animal ready to spring upon its prey, the startled animal that seeks safety in motionlessness, Darwin's drawings of the fawning cat and the angry dog, The Dying Gladiator, a child at prayer—in all of these and countless others the 'frozen' phase of the total movement is all that is necessary for expression, and it does not make the least difference by what movements the posture is reached or what movement will follow.[11]

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[10] R. Magnus (12, pp. 339-553).

These three fundamental conditions of the postural expression, the attitude of the observer, the situation in which the posture is placed, and the nature of the posture itself, are curiously interdependent. It will be noticed that if the situation is given it may in part determine the attitude, and if it is not given it may in part be determined by the attitude. The posture, on the other hand, may in part determine both the attitude and the setting, and the expression of the posture depends upon the attitude and the situation (which includes the posture).

The Apprehension of the Posture. When the observer is confronted with a posture under the instruction to find its expression, his response may be immediate or delayed. The conditions which underlie the apprehension of the expression will naturally differ in the two cases, and our exposition will be simplified if we distinguish two forms of postural expression—the innate and the conventionalized posture.[12] The former may be defined as that posture which an animal (according to its species) assumes in a particular situation, as, for example, when confronting its prey, or when avoiding danger. The conventional posture is an acquired posture; it is one that the social group has selected and employs with the intention of expressing a particular meaning. For example, the child on bended knees with bowed head and folded hands by a convention of Christian peoples means 'prayer'; the baseball umpire with bowed trunk and head, arms extended, and palms down by a convention of the devotees of the game means 'the runner is safe.' Since the conventional posture is employed with the intent to express, it is a symbol, exactly as a letter or word is a symbol, and its meaning must be known before that meaning is expressed. The innate posture may, of course, become conventionalized and in the human species may be and has been assumed with the intent to express. The conventional posture, on the other hand, may become so habitual as to appear to be innate. For example, the kiss as a sign of affection, and the clenched fist as a sign of anger are modes of behavior so frequent among Western peoples as to appear to be innate, but, as Darwin has shown, neither is common to all social groups. In the human species it is often impossible therefore to say whether a given posture is innate or conventional.[13]

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[11] Socrates remarks: "Nobility and dignity, self-abasement and servility, prudence and understanding, insolence and vulgarity are reflected in the face and in the attitudes of the body *whether still or in motion*" Xnephon (17, p. 5. Marchand's translation, italics ours).

[12] The innate and conventional posture are to be distinguished from the natural and unnatural posture mentioned above, p. 415, and both types of posture from the nativistic and empirical expressions mentioned below in note 13.

If the posture is conventional and is known, or if it is innate but familiar, its expression may be directly apprehended; it is as immediate for perception as is the relative size or the sex of the figure. The expression in such cases comes 'in a flash,' 'is there at once,' and the observer is unable to note any of its conditions. If, on the other hand, the posture is new, strange, or unnatural, the observer may not be able, under our instructions, to find any meaning other than 'unnatural,' 'incongruous,' and the like. In our experiment, however, there were many postures which either differed slightly from familiar postures, or were unusual, or unknown, and yet appeared 'natural.' The expression of these postures was not immediate, but delayed, and the observer tried to discover the expression. Various devices employed in this search have already been indicated. We have seen that in some instances some aspect of the total posture that was immediately significant caught attention and the meaning of this part was extended to the total posture. At times the posture was analyzed in the hope of finding some aspect that would supply a meaning. At other times the attempt was made to fit the posture into a situation, or to discover what it was that the posture was doing. We have seen also that there is a tendency for the observer to imitate in his own person the posture visually perceived and to base his report upon what he felt rather than, or in addition to, what he saw; he may even, as in empathy, project his own feeling into that of the seen figure. Finally we have seen that with these devices the observer employs all his psychological resources—memory, imagination, action, thought. Whatever expression he finds, however, he is, under the instruction of our experiment, still confronted with the task of reporting verbally the expression found.

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[13] Darwin (2, pp. 216, 220, 247, 257, 307, 353). See, however, Mantegazza (6, p. 80) who assumes that the kiss and the clenched fist are "spontaneous automatic expressions" and, among others, are universal modes of expression constituting "a veritable universal language." Most of the earlier writers seem to have assumed that the physiologically innate gestures, facial and bodily postures are also as expressions 'nativistic.' This view was vigorously assailed as early as 1865 by Le Moine (11, pp. 88, 106, 162, 212) who says, "It is the experience of life commencing in the cradle that teaches us to read on the faces of our fellow men the passions of their minds." Köhler (8, pp. 233 ff.), if we understand him, seems recently to advocate the older view. The problem involved is, of course, a genetic one and lies beyond the scope of this study.

The Verbal Report. When the posture is taken as a phase of movement there is usually no difficulty in finding words to characterize the movement. When, however, it is regarded as the expression of a mental state or function, the finding of the fitting word is often exceedingly difficult, not necessarily because the observer is at a loss as regards the significance of the posture, although, as we have just seen, he may be; his difficulty in this context in purely verbal. If the expression is conventionalized, and if the conventional posture is known, the fitting word is immediately supplied upon the presentation of the posture. If, on the other hand, the posture is suitable to a number of situations any one of which would give the posture a slight difference in meaning, it is not always easy to find the word that fits the posture in a particular situation. Furthermore, even if we could assume that the attitude of all observers is the same, and the situation in which the posture is placed is also the same, we should still expect to find slight disagreements among the observers as regards the fitting word. The reason is due in part to the ordinary difficulty of finding the best word, and in part to the fact that the fundamental postural expression is general and not specific. We have seen (p. 422) that, given the same attitude and similar situations, the terms employed by our observers were similar. In some cases they were synonymous, in others they seemed to differ only in degree, but even where logical differences may easily be found they still are similar, they belong together as species to a genus. We have therefore distinguished a generic expression to which the specific expressions belong.

The Generic Expression. Upon the presentation of a posture under our conditions the observer has no difficulty in saying in a general way what is signified. He employs such phrases as "the figure is withdrawing from some object or situation," "he is approaching some object or situation," "the expression is of a general state of well-being," "the figure is able to cope with the situation," "the concern is not with some external object but with his own thoughts." It is obvious that every one of these general expressions might be further specified by particular words; consequently, we have referred to them as generic expressions.[14] With the aid of the observer's reports we have classified them into four kinds and, with some hesitation, have designated them as approach, withdrawal, expansion, and contraction. The first two are obviously behavioral, and they suggest the positive and negative reactions of the unicellular animal. But they are not so simple and they do not necessarily imply movement. The term *approach* does not solely mean that the figure is itself approaching an object, for the object may be, as when the figures 'offers' or 'receives,' invited to approach the figure. The term is also generic for attention to, interest in, or scrutiny of, an external object. Etymologically, of course, the term 'attention' means 'stretch towards,' and the forward posture as expression of the figure's interest or curiosity is literally giving itself up to the object attended to or scrutinized.[15] Similarly, *withdrawal* does not always mean that the figure is itself moving away from the object; it may, with arms forward and palms out, be pushing away the object. The characteristic posture is either a drawing back or a turning away from the object; consequently, it indicates negation, refusal, repulsion, disgust, and the like.[16] At times, particularly with a backward trunk and head in which the chest is naturally raised, the withdrawal is mixed with *expansion* in a proud refusal, dramatic negation, horror, etc. The term *expansion* refers behaviorally to the expanded chest and also to the erect or backward trunk, erect head and raised shoulders, which add height to the figure. It is generic for such states as pride, conceit, arrogance, disdain, mastery, self-esteem, and their synonyms. There are many figures of speech and words which derive from the expansive posture; such, for example, as puffed up or bloated with pride, swollen with conceit (both of which may refer also to the heart), turgid, inflated, 'a swell,' 'assumes a lofty bearing, haut, haughty, exalted, high spirits, etc.[17] *Contraction* is characterized posturally by the forward trunk, bowed head, drooping shoulders, and sunken chest. The figure shrinks, shrivels, cringes, cowers, crouches, droops, and thus becomes an expression for that large group of mental states or functions in which the individual withdraws into himself, is depressed, abased, downcast, dejected, crest-fallen, and the like.[18]

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[14] Mantegazza refers to them as 'relations' (14, pp. 188 f.). See also Darwin (4, pp. 246, 254, 262) and Bell (3, p. 168).

[15] See Aubert (2, p. 98), Mantegazza (14, p. 205), Köhler (8, pp. 250 f.), in which both inner and outer states are characterized as 'tension.'

[16] See Aubert (2, p. 20) and Darwin (4, pp. 256-261).

[17] "It is not without reason," writes Mantegazza (14, p. 117), "that in every language inflated signifies proud, and to inflate one's self to grow proud." See also Aubert (2, pp. 35, 113); Darwin (4, pp. 246, 263 f.).

These generic expressions are not unequivocal. They are in part conditioned on the attitude of the observer; one and the same posture may be an approach *or* a contraction, a withdrawal *or* expansion, and the situation in which the posture appears may, of course, determine the attitude.

Specific Meanings. Since the generic expression includes a large number of specific expressions, any one of which is suited to the posture, the question remains as regards the determinants of the specific expression. As we have already seen, the principal determinant in our experiment was the position of the arms and more particularly of the hands. These gestures, as Table 4 shows, separate the expressions which belong to the generic posture into smaller groups. We may take, as a single example, the approach of Posture 1, Table 4; with palms up the approach becomes acceptance, offering, coaxing, supplication, beseeching, all—with an humbleness which derives from the posture as a whole; with palms outward, active repulsion, avoidance, opposition, command, disapproval; and with palms down, soothing, calming, blessing. It is probable that the facial expression would have also served as a specifying factor. But in our experiment the arms and hands did no more than to indicate a number of terms any one of which was suitable to that particular gesture in connection with the basic posture. Why, then, did the observers choose the one or the other of these particular terms? In the first place, the term which the observer furnishes is conditioned upon his vocabulary. His associations are, of course, constrained first by the generic meaning and secondly by the gesture. But otherwise they are free and, we may suppose, depend upon the laws of association. At times the 'fitting' word does not come, and at other times the observer furnishes two or three terms any one of which seems fitting. There is no doubt but that among observers there are differences as regards the felt necessity of finding the most suitable word. We have attempted no quantitative study of the relative agreement of observers concerning the word employed as indicative of the expression found. Statistical studies of facial expression have shown that the agreement among observers is low, and we have no reason for believing that a similar investigation of bodily expression would give a different result. Indeed, when the wide range of conditions which underlie postural expression are considered, one wonders that under the artificial conditions of experimentation there is any agreement at all. That there is some agreement under these conditions is largely due to the conventional or familiar postures to which a single word has been attached. We have been forced to the conclusion that, as a rule, the postural expression is not specific but general, and a similar result is to be found in recent studies of facial expression. Frois-Wittmann for example shows that judgments of a single facial pattern are distributed about a single term as the mode, and judgments of certain similar patterns are by frequency of occurrence related. Such a relation occurs, for instance, among sneering, disgust, contempt, and hate. We cannot doubt but that, if a single general term could be found which would include these specific terms, or if the observer had been allowed to say in general terms what the pattern signified, agreements, with differences in attitude aside, would have been almost if not quite complete. In everyday life and even on the stage and in painting, we may suppose that the generic expression is practically unequivocal because to the posture is added both the facial expression and the situation which includes the posture—the latter being sufficient in most cases to determine the attitude.[19]

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[18] Mantegazza (14, pp. 253 f.) says, "the expression of pleasure is always centrifugal and that of pain centripetal." He employs the term 'expansion' in the sense of a diffusion of muscular excitation in the order face, neck, arms and hands, trunk, and lower limbs, which, he thinks, is correlated with successive degrees of intensity of emotion. In another context he seems (p. 253) to use the term expansive to mean expressive. See also pp. 185 ff.

Summary

Observations of photographs of a large number of bodily postures systematically selected and presented with an instruction to report the expressive value of the posture, and, if possible, to designate the factor most significant for the expression given, indicated that:

1. Bodily posture was expressive under the conditions of attitude, situation, and the nature of the posture itself.

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[19] This question of the generic and specific nature of the postural expression needs further study. The generic was reported by our observers with less difficulty and greater certainty; it seemed to be more immediate, more intimately bound up with the posture, and less dependent upon the situational context than did the specific expression.

2. Postures were taken under two points of view, first as expression of an attitude or emotion, and, secondly, as expression of a movement.

3. Under our conditions there was no guarantee as to the way in which the posture would be taken. Some of the postures, however, were always taken under the first view, and others under either view.

4. The situation may determine the attitude of the observer toward the posture.

5. The postural expressions found may be subsumed under two categories, the generic and the specific. These are related to each other as genus and species.

6. The postures were taken as a total, but the observers were usually able to indicate one factor of the posture as most significant for the expression given.

7. The head and trunk taken together are basic to the postural expression; arms, hands, distribution of weight, etc., are to make more specific the more general expression of the head and trunk.

8. One factor of the posture may become focal and give to the total posture an expression carried by this factor.

9. Each series of postures based upon a certain factor of the total posture gives a group of related expressions.

10. If two observers focalized different factors of the same posture, different expressions were given. This fact, together with differences in attitude, differences in the situation provided by the observer, differences in vocabulary, and differences in the degree to which a posture is associated to a particular word, accounts for the verbal disagreements which occurred.

Reports made under instructions to describe the experiences of the observer while finding the expression showed:

11. The expressions of familiar postures were immediate and no concomitant processes were observed.

12. Relatively unfamiliar postures were accompanied by memorial and imaginal constructions, empathy, inferential processes, and tendencies to imitate the postures presented.

13. When the observer was asked actually to imitate the posture and to report the expression of the posture felt, the reports, in general, agreed with those based upon the posture as seen, but the agreement was not invariable.

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UNE ÉTUDE DE L'EXPRESSION DE LA POSTURE DU CORPS

(Résumé)

On a projeté sur un écran des photographies d'un mannequin en 347 postures différentes du corps et on a demandé à l'observateur de dire dans une série d'expériences ce que la posture exprimait, et dans une autre série de rapporter les idées et les émotions que l'observateur lui-même a eues, et dans une troisième série d'imiter la posture vue et de rapporter la signification de la posture éprouvée. Les résultats montrent que l'expression est conditionée par (1) une attitude (*Einstellung*) de l'observateur, c'est-à-dire, la posture peut être prise comme une expression d'une fonction ou d'un état mental, ou elle peut être prise comme une phase d'un mouvement; (2) une situation que fournit l'observateur, si l'expérimentateur ne la fournit pas; (3) la nature de la posture elle-même. Ces trois conditions sont cependant dépendantes l'une de l'autre. Puisque dans nos conditions l'attitude et la situation sont fournies par l'observateur, nulle posture n'est équivoque dans

son expression. Les expressions spécifiques rapportées sont des espèces d'une expression plus générique. On a classifié celles-ci comme Approche, Eloignement, Expansion, et Contraction. Chaque expression spécifique rapportée, sauf celles d'un mouvement, peut être subordonnée à une de ces quatre catégories, mais il faut s'attendre à des variations parmi les observateurs à l'égard du mot spécial choisi pour caractériser l'expression. Cela est vrai à cause des différences de vocabulaire, de la difficulté de trouver le meilleur mot, du degré relatif auquel la signification de la posture est familière, des petites différences dans la situation fournie par l'observateur, et enfin des différences avec lesquelles on s'appuie sur de certaines parties de la posture totale. Quant à celle-ci, la position du tronc, et celle de la tête qui la suit naturellement, sont les plus importantes pour l'expression générique. La position des bras et de la main est signifiante pour les expressions plus spécifiques. La distribution du poids du corps a rapport à la position du tronc. Des expériences où l'on a demandé à l'observateur d'imiter la posture et de rapporter l'expression éprouvée ont montré une corrélation élevée avec les rapports de l'expression de la posture vue. JAMES

EINE UNTERSUCHUNG DES AUSDRUCKS DER KÖRPERLICHEN HALTUNG

(Referat)

Es wurden Photographien einer Gliederpuppe in 347 verschiedenen Haltungen kinematographisch dargeboten, und der Beobachter wurde ersucht, in einer Versuchsserie die Bedeutung der Haltung anzugeben und in einer zweiten Serie seine eignen Ideen und Empfindungen bei der Beobachtung anzugeben. Die Befunde erwiesen, dass der durch die Haltung gegebene Ausdruck bedingt wird durch: (1) eine Einstellung des Beobachters (d.h., man kann die Haltung als Äusserung einer geistigen Funktion oder eines geistigen Zustandes betrachten oder als eine Phase einer Bewegung); (2) eine Situation die, wenn sie nicht durch den Versuchsleiter gegeben wird, von dem Beobachter verschaffen wird; (3) die Beschaffenheit der Haltung selber. Diese drei Bedingungen sind aber von einander abhängig. Da unter den gegebenen Bedingungen die Einstellung und die Situation von dem Beobachter verschaffen werden, ist keine Haltung unzweideutig zu deuten. Die spezifische Ausdrücke die gemeldet werden sind Gattungen einer mehr generischen Ausdrucksform. Letztere haben wir klassifiziert als: (*a*) Annäherung, (*b*) Zurückziehung, (*c*) Ausdehnung und (*d*) Zusammenziehung. Jeder spezifische Ausdruck der gemeldet wurde—mit Ausnahme der Ausdrücke einer Bewegung—können einer dieser vier Kategorien untergeordnet werden, aber Variationen unter den Beobachtern in Bezug auf das besondere Wort welches zur Karakterisierung des Ausdrucks gewählt wurde sind zu erwarten. Diese Variirung entsteht aus Verschiedenheiten des Wortschatzes, aus der Schwierigkeit der Entdeckung des geeigneten Wortes, aus der relativen Bekanntheit der Haltung in Bezug auf ihre Bedeutung, aus kleinen Unterschieden in der von dem Beobachter verschaffenen Situation, und endlich aus Unterschieden in dem Nachdruck auf gewisse Teile der gesammten Haltung. Von diesen Teilen sind die Haltung des Rumpfes, und die des Kopfes, die naturgemäss folgt, für den generischen Ausdrucksform am wichtigsten. Die Stellungen der Arme und Hände sind für den spezifischen Ausdruck von Bedeutung. Die Verteilung des Körpergewichts steht zur Haltung des Rumpfes in Beziehung. Versuche, in denen der Beobachter ersucht wurde, die Haltung nachzunahmen und über den Ausdruck so wie er empfunden wurde Bericht zu erstatten, ergaben eine hohe Korrelation mit den Berichten über den Ausdruck so wie er gesehen wurde. JAMES

CONFIGURATIONAL FACTORS IN THE UNDERSTANDING OF ACTIONS*

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I. Statement of Problem

It is a familiar doctrine of Gestalt psychology that our perceptions manifest themselves as total organized patterns or configurations. In the field of *static* visual figures of a geometrical character Gottschaldt (1) has convincingly shown that, despite the large amount of preliminary practice, the recognition of simple overlearned forms is well-nigh impossible if the environment or the organization of the total field be sufficiently changed.

Professor Köhler proposed that the writer investigate whether the understanding of the meaning of an observed act lies in the comprehension of the structure of an event, and suggested that the answer to this question be sought as follows: Specific incomplete actions could be executed *once* in a spatial and temporal environment which strongly favored the development of the appropriate corresponding phenomenal "occurrence-structure" in the observer, and then *again* performed in a total setting which facilitated "false" structuration or closure. There is, of course, an obvious analogy here with the facts of camouflage in the perception of objects. It was anticipated that in the first case the understanding and the consequent correct ideational supplement would be more readily forthcoming than in the second instance. Considerations such as these led the present writer to plan the following experiments which, though they may not represent perfectly the precise question at issue, may be viewed as a first step in the solution of the above problem.

Our everyday experience tends to support the affirmative opinion. An identical act, e.g., wiping a tear from the eye, has one meaning when it occurs in the spectator of a stage tragedy and another on a windy road. Since most actions of this sort bear a pronounced movement character we may speak in such cases of *dynamic* gestalten to

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distinguish them roughly and tentatively from the other varieties. One sees here more plainly than elsewhere the close alliance between perceptual performance and general intelligence. Köhler has expressed himself in this connection as follows:

> "*Understanding* or *comprehension* is the first important kind of intelligent behavior. What a radiant smile passes over the face of a little child who has just grasped the meaning of a game which an adult has begun with him! The sudden apprehension of a connection in an entirely different direction than was previously to be expected is the functional kernel in the understanding of many a good anecdote; a smirk or Homeric laughter generally reveals what has happened, but to be effective it must refer to something very definite, as we may observe in ourselves whenever we wait, embarrassed and in vain, for the anticipated to occur" (2, p. 133).

Our hypothesis is that, just as things or figures exhibit compulsive organizations, so successive activities may be firmly integrated. However, a special distinction between the two is easily seen in the following state of affairs: Movements are not just simply "there," but they indicate a direction, they are going somewhere, they point to something definite. All movements, therefore, possess a vectorial feature. One can observe this vector especially clearly in any *unfinished* action, e.g., when watching a pole-vaulter running with his pole toward the objective one expects to see a vertically directed leap and not the runner suddenly pause and sit down. The spectator places all the physical items and each phase in the course of an action in juxtaposition with one another. This implies that, in order to understand any action, one always takes into consideration the *total situation.*

After these preliminary remarks we may formulate the subject of our investigation more exactly in these terms: "What influence does the objective constitution of the visual field have upon the understanding of an interrupted action?"

II. Method and Experimental Procedure

In the experiments of this investigation not all the partial units of behavior which belonged to a total action were completely performed, i.e., if a total act consisted of ten sub-acts, then only a fraction of this number, comprising perhaps three to seven stages, was completed. During the experiment the subject was merely a passive spectator of the executed actions. In his phenomenal experience there would normally arise an urge toward the completion of the act as though a gap were present which needed to be filled. Under certain conditions the subject can tell without any difficulty what is necessary to the fulfilment of the action. There are other circumstances, however, in which the subject is unable to determine either the direction or the nature of the action still to occur.

All the experiments, to be described below, took place in a moderately large, light room. Each situation was presented individually; no subject had the least notion concerning the "point" of the study. The following instructions were read: "During this experiment pay maximum attention to everything I do. Your main task is to determine the intention which lies back of my behavior. (The experimenter carried through all the acts himself.) Watch your phenomenal field and note when and how your ideas arise. Distinguish, please, between your first naïve impressions and any later analyses."

Before each experiment began the subject was required to list the visible items in the room in order to be sure that nothing essential was overlooked; in this way the attention factor was controlled. After this descriptive inventory the experimenter said, "Now watch carefully what I do." He then began to speak, move about, and manipulate things in a manner as lifelike and uneventful as the simple situations simulated. The minimum of dramatization required was regulated by holding the rhythm and accent of the speaking voice, gestures, etc., as constant as possible with all subjects. The place at which the interruption in the chain of acts occurred was rather arbitrarily determined—whether early or late in the process depended upon the complexity and length of the total action. As soon as the experimenter had suddenly broken off the act, he returned to his other rôle and asked, "What did I intend to do?" A stenographic record of the subject's report was immediately taken.

Between each experimental situation the subject waited in a vestibule adjoining the laboratory room and returned after a necessary brief interval to a newly prepared situation. During the entire experiment the subject remained seated at the work table opposite the experimenter's chair. Unless otherwise specifically mentioned in the following description, the objects always rest upon this table and are itemized sequentially in a left-to-right order—left here referring to the experimenter's left. Ten simple situations were constructed:

1. A picture lies in an accidental position upon the floor, about ½ m. from the wall. The experimenter's topcoat has been casually

thrown upon a sofa, whose foot end is one meter distant from the picture. In the middle of the room is a long table, upon which are strewn nails, wire, and wooden blocks of three different sizes; a few centimeters to one side of this mass lies a coathook with two holes in which two screws have already been inserted.

Action: The experimenter chooses the largest nail, the largest piece of wire and the biggest block of wood, moving each object in a critical manner through the three dimensions of space before he removes it, and then places all three upon the couch between the coat and the picture. (Intention: to hang up the picture.)

2. Upon the work table rest the following objects: left, a torn colored map of Europe; right, a fair-sized piece of stiff yellow paper, upon which small paper strips have been pasted to form geometrical figures—one is a complete triangle, the other a hexagon with two sides missing. In the middle of the table are scissors, ruler, a tube of paste, and white paper of the same kind as the paper strips described.

Action: The experimenter sits in his chair and soliloquizes, "The length is about 14 cm.," and slowly cuts out two equal paper strips, whose dimensions correspond to the strips forming the figure (but also to the length of the rent in the map). After the experimenter has smeared the surface of one strip with some paste, he breaks off the action. (Intention: To repair the map.)

3. A large sign with the unfinished phrase "NO ADMI —" rests upon the window sill. On the table is a bottle of black india ink, a used brush, a sheet of paper with a few visible brush strokes, and a letter upon which stands a small wooden pyramid as a paper weight.

Action: The experimenter says, "What shall I be able to finish today?" and slowly lifts the letter from its position. He reads aloud: "This block cannot be used for experimental purposes since the grain is visible. What shall be done about it?—(Signed) Assistant." The experimenter returns the note to its original position under the pyramid and says, "Stupid fellow! Can't he take care of that himself?" He goes to the ink bottle, dips the brush, scratches upon the surface of the paper, and remarks, "I think that will do; the ink still appears to be thick enough to work with." (Intention: To paint the pyramid.)

4. In a semicircle on the table lie a safety razor and shaving brush, a honing stone, a razor blade, an unpointed pencil, a folded sheet of white paper, a small piece of cloth or dress goods.

Action: "The experimenter directs his eyes upon the razor blade and asks, "Will I be able to do the job with this or not? There is nothing else I can use." He picks up the blade, sharpens the edge upon the stone and touches it gently with his fingertip. He then returns it to its original position, meanwhile commenting, "That will have to do." (Intention: To sharpen the pencil.)

5. Simple geometrical paper figures (rhombus, star, etc.), the sheet out of which they have been cut, a scissors, an open knife, a daily paper and two political clippings lie on the table side by side.

Action: The experimenter speaks: "Should I use the knife or the scissors for this purpose?" and tests them by making a few incisions in the paper sheet. As he pushes the scissors to one side he exclaims, "I believe the scissors will be better for this." (Intention: to clip more from the newspaper.)

6. An open technical magazine, upon whose visible pages a curve can be seen, rests upon the table. Upon some writing paper containing a hasty pencil and ink sketch reproducing the original graph, lies a ruler with a metal edge; close beside it a pencil.

Action: The experimenetr lifts the pencil and busily reads a page in the journal. He thumbs through it and finds that some pages have not been cut, thrusting his forefinger in to verify this. He then drops the pencil and magazine, picks up the ruler, examines its edges, and finally returns it to its original place. (Intention: To cut open the pages with the ruler.)

7. On the table are found a cardboard box wrapped in stout paper, but untied; three simple strong knots laid side by side; a ball of twine; four pairs of wooden sticks, two of which are tied together and two only placed in contact; a new red book.

Action: The experimenter takes a pocket knife from his vest, cuts off a piece of cord about 2/3 m. long and twists it around the length and breadth of the new book. Before he joins the ends in a knot, he stops suddenly and exclaims: "My, how absent-minded I am!" He removes the string and places it near the wooden blocks. (Intention: To pack the book in the box and then tie the parcel.)

8. Calligraphic model with alphabet, scratch paper, a valuable pen; a bill and a blank check.

Action: The experimenter takes the pen in his hand and makes a few wavy lines upon the writing paper, and then signs his name underneath, meanwhile saying, "This is a very important matter and I'd better do it carefully." (Intention: To write the name under the model in large attractive handwriting.)

9. A pair of shoes upon the window sill; a large stiff kitchen-brush and a soft shoe-brush; a very dusty Quincke apparatus.

Action: The experimenter runs his fingertips over the iron tubes and then rubs his hands. He takes the shoe-brush, runs his fingers through the bristles and replaces it. (Intention: To clean the apparatus with the soft brush.)

10. At the right end of the table stand three glasses, unequally filled with dirty water; a sponge; chalk. Leaning against the wall, 2/3 m. away, stands a blackboard covered with writing.

Action: The experimenter dips the sponge in the glass with the most water and squeezes it out in the one with the least. The sponge is left remaining on top of the glasses. (Intention: To equalize the water level in all glasses.)

If one assumes that the visual configuration is the decisive factor in the comprehension of a situation, one can readily determine to what extent the meaning is altered by an arbitrary destruction or strengthening of the coherence of the members in a gestalt. An objective constellation which is changed suffices to produce a shift of meaning in a different direction by modifying the meaning of another factor present.

Situations 1 to 5 were performed before ten subjects constituting the A-Group of the study. Since there can be no positive interpretation of the partial act under the above conditions we may call these the "ambiguous" situations. In the main, there are but two significant possibilities (although more may occur) and the total state of affairs has a pronounced "either-or" character.

These same situations can be made "clear" and "obvious" through the omission of an opposing factor, which procedure was actually undertaken with another group of ten subjects constituting the B-Group of the investigation. The method is very simple; the action then appears natural and unambiguous and can be brought to a meaningful close in a *single* direction only. In this series we removed several articles in each situation according to the schedule below:

Situation 1: coathook with topcoat
Situation 2: sheet with pasted paper strips
Situation 3: sign
Situation 4: safety razor, brush, a piece of white paper, a piece of cloth
Situation 5: paper figures

According to our hypothesis the A-Group should *never* reach an insight into the intention of the experimenter. But this ideal result is not easy to attain, since there are usually two apparent possibilities, and, consequently, probability alone (which is not applicable here in its pure form) would permit the A-Group to reveal a correct understanding in 50% of the cases. The conditions with the other group are practically reversed: should the results correspond to our ideal expectation, one should secure 100% accuracy in the insight into the action to be finished, since the total behavior moves plainly in just *one* direction.

In order to equate the differences in intelligence and other unknown attributes of the subjects, Group B later was given the situations 6-10 in the "ambiguous" version and Group A in the "obvious" variation. The following objects were *not* presented to the last group:

Situation 6: drawing paper
Situation 7: cardboard box
Situation 8: bill and check
Situation 9: shoes
Situation 10: blackboard

The subjective intention of the experimenter, of course, remained the same in both the ambiguous and the obvious situations.

III. Results

The principal results of the investigation are quantitatively summarized in Table 1. The figures indicate what proportion of the subjects were able to discern the intention of the experimenter from his introductory behavior and its relation to the objects visible.

One must avoid drawing definite but premature conclusions from these figures. The responses recorded in the protocols were not easily cast into quantitative form, but the process of interpretation was simplified by taking the *first* naïve utterances of the subjects as the criterion of the strength of the vectors involved. Reflection enables many subjects to hit upon the correct answer later, but such cases have not been included in the tabular data above.

In general, one observes a clear superiority of the "obvious" situations since the correct solutions occur here much more frequently. The difference would probably be considerably larger were it possible to disentangle those solutions among the ambiguous cases

TABLE 1

Percentage of Subjects Who Rightly Determined the Actual Intent of the Experimenter in "Obvious" and "Ambiguous" Situations, Where the Sole Difference Lay in the Absence of Certain Objects

Situation No.	Obvious Group (A)	Ambiguous Group (B)
1	50%	10%
2	100	0
3	100	40
4	100	20
5	80	20
	Obvious Group (B)	Ambiguous Group (A)
6	87.5%	70%
7	70	12.5
8	75	0
9	100	80
10	50	0
A.M.	81.25	25.25
M.V.	16.25	22.85

which were correct only through guessing. However, even without special consideration of these cases the average difference between the two groups is quite large, although the dispersion still remains noticeably high.

The results are to be judged only as approximations to the theoretically expected, since only in *one* case (viz., the second) did the experiment succeed perfectly. In this ideal instance the experimenter *intended* to repair a map which had a 20-cm. rent in it; but the fact that an incompleted figure composed of strips of the same length was prominently displayed in the same visual field drew the observer irresistibly in this direction. Here the distracting "disturbance" was very effective. An analogous rôle to the figures made of paper strips is played by the shoes in the ninth situation. But they embody an ineffectual vector. Presumably the remarkably dusty steel apparatus affected the experience of the subjects so emphatically that the shoes, which appeared relatively shiny beside it, retreated into the background of the stream of consciousness.

These considerations do not involve the rejection of our hypothesis but merely demonstrate the existence of more and less firmly organized structural complexes.

In order to portray the behavior of the subjects more vividly, it may be profitable to reproduce a few passages from the protocols. The qualitative differences in the reports under the different conditions are more enlightening than the pure figures of the table above. Many subjects answered with positive assurance; they were convinced that they knew the intention of the experimenter—an indication of the extraordinary strength of many of the vectors!

Subject Ma (B-Group). Ambiguous cases.

Ad Situation 1. "It occurs to me that you may plan to hammer the coathook into the wall." (Said after a long pause.)

Ad 2. "You meant to finish this thing!" (Pointing to the incomplete figure. Immediate natural response.) . . . "You could, however, repair the map with it."

3. "You wanted to paint the sign, but you lacked enthusiasm for the task."

4. "You wished to shave yourself . . . . Hold! A stupid idea, since one doesn't shave in such an environment. If the experimenter were very dull then the shaving would be the most probable, but this solution does not satisfy me."

5. "I hover between two possibilities: You may wish to make some new figures, or . . . No, the whole thing is too muddled for me."

(Obvious cases.)

6. "I thought, at first, that you planned to read and take notes, but when I saw that the pages were uncut I saw at once that the ruler was an appropriate tool for this purpose. You could have broken off the action earlier; I could have prophesied your next movement."

7. "It is possible that the matter has something to do with bookbinding, but that strikes me as meaningless. The wood blocks and the book do not fit together."

8. "To write an essay or some application."

9. "You wished to dust off the 'Hornbostel' and first tested the brushes. You will probably not use the brushes since they are apparently too good for this purpose!"

10. "The sponge was used to transport the water in order to bring about the same level in all the glasses. Still, why didn't you pour it from one to the other? . . . Perhaps there is a board somewhere, and you wished to dampen the sponge; or it is possible that you merely intended cleaning the sponge itself."

This last instance is especially interesting since the first correct solution was not satisfactory because it did not seem meaningful enough. Of great significance is the urge toward the "sensible"; the subject sought a blackboard which was not present because it stood in better connection with the remaining objects!

For comparison, we may take a subject of the A-Group where the serial order was reversed. The similarities and oppositions show an interesting counterplay, if one reads the sentences in parallel.

Subject Wi (A-Group). Obvious cases.

Ad Situation 1. "You could hang the picture up with these tools; but I am not completely certain of it."

Ad 2. "To paste this rip together—everything moves in that direction. Every single phase of the activity is connected with the goal."

3. "In order to remove the visible wood grain by means of painting the surface. The articles are not exactly appropriate, but the whole thing came together very quickly."

4. "To sharpen the pencil. The tendency was there at the very start. It would not have been necessary for you to have done anything. The action was neutral since everything was clear from the start. The matter is so primitive, that there is no other escape."

5. "You wanted to make further clippings in the newspaper. You have already made two."

(Ambiguous cases.)

6. "Should I give you a knife? (laughingly uttered as the subject observed that the sides were not cut). You wished to read further to see if the ruler would do the job. . . The idea came quite spontaneously, at any rate in non-intellectual fashion. The connection was simply there—necessarily so. No difficulty. When the ruler and the uncut pages came optically together, everything tumbled, and the relationship was unified."

7. "You probably intended to tie up the book, but you did not do it properly."

8. "You wished to sign your name carefully in order that the person who had sent the bill would have no trouble converting it into cash. The bill and the check were together right at the beginning, before the action developed . . . But the ABC doesn't fit into this aggregate of objects; I have no notion what to do with it."

9. "You acted as though you wished to remove the dust from the apparatus. The soft brush does not please you for some unknown reason."

10. "Entirely dark. General tension, but no pressure in any special direction."

The most interesting example in the case of this subject is Situation 4, where the entire action was seen in advance. Such cases occur rather often and show how intimately some movements are associated with certain objects or object-constellations. A knife, e.g., indicates at once the natural and related function of "cutting," despite the fact that it can also be used to scratch one's self.

IV. Discussion of Results

As the numbers in the table suggest, the individual differences in the apprehension of the situations are very marked. The variations in the manner of understanding could perhaps be explained by the lack of uniformity in personal experiences, but this is a dubious account, particularly when one knows little or nothing concerning the nature of this presumptive experience. The *mental set* of the subject is probably much more significant than his experience and, in addition, is much more readily tested. After one, or at the most two, situations, the subject becomes aware of the pattern of the experiment and tends to build hypotheses with respect to the future course of the action before it has been executed. This factor of *Einstellung*, then, may well be the major disturbing cause influencing the outcome.

The total experimental situation is also influential; to many subjects the actions appeared remote from reality and genuinely funny. Such persons remained keenly alive to the unnaturalness of the situations represented and declared with passion, "But you never did have any actual immanent purpose! You simply wanted to know what I would say in response to your behavior (as a test of my attentiveness, intelligence, memory, etc.)." Although the subjects knew full well that the actions were purely histrionic, many of them offered great resistance to specific "vectors," believing that only a feebleminded or pathological person could be guilty of them in real life. This fact is noteworthy since it confirms something which the results of memory work with nonsense syllables has shown so strongly, viz., that the normal human being resists submission to the unclear, the indefinite, and the wholly irrational. It would seem as though the mystical and the incomprehensible in everyday life affect the ordinary man not as something to be admired, but rather as something uncomfortable. If in the composition of the exposed articles some object necessary for the understanding of the witnessed activity be missing, the entire happening loses its significance for the subject.

This "understanding" of the act need not always be a rational knowing; the subject experiences the closure or lack of closure of an action as concordant or discordant. This feeling of inadequacy or incompletion releases energies which struggle for a better organization.[1]

It is also plausible that graduate students are not the best material for an experiment of this nature. Their associations are often surprisingly rich and they border on the fantastic. (One subject watching Situation 8 said the experimenter must be a Russian unfamiliar with the Latin alphabet who could only write a letter with the aid of a model!) It would be profitable to carry through an analogous study with children or relatively untrained subjects to see if, as anticipated, such groups reveal sharper differences in the "clear" and the "ambiguous" situations.

V. Theoretical Conclusions

The fundamental problem implicit in this investigation is: Can understanding be identified with awareness of the specific structure of an experience? If that be the case then the experience of observing an action must approach or recede from the real intention of the actor to the extent to which all the factors involved converge upon or diverge from the corresponding organization. That such a relationship actually obtains seems likely from the evidence presented in this report, even when full recognition is granted all the weaknesses of a tentative and exploratory method.

We have dealt here with what seems to be the most primitive and yet most general stage of the phenomenon of insight. The whole field of imitation and suggestion which has so frequently been misconstrued may acquire a new significance and interpretation when seen in this light; and even the much despised topic of intuition may need to be re-evaluated. Are women really more competent than men in the field of intuition? If that be so—and it is not asserted that such is the case—it does not necessarily imply the existence of a purely feminine capacity, but may merely indicate a stronger preference for perception according to the principles of totality as opposed to purely analytic receptivity. So-called social intelligence, whose most essential component appears to be tact, is also intimately related to this topic.

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[1] See in this connection the later writings of Felix Krueger, especially (3).

In connection with a striking example, Köhler (2, p. 143)[2] has elaborated an hypothesis which has some claim to universal validity, as the quotation translated below suggests:

> "Suppose one steps before an audience and promises to demonstrate the physical effectiveness of maximum concentration of the will. A stick will, although only laterally in contact with the flattened hand, remain swaying free in space, provided the requisite voluntary energy conducted through both arms suspends the operation of gravity. Then the demonstrator with the stick in his left hand and his right hand pressed about the wrist of the left, turns toward the right (left of the spectators), raises his arms and the stick, simulates extreme effort by means of appropriate gestures and through tetanic innervation of all the arm muscles until they tremble, and then slowly opens the hand in which the stick lies until the hand forms a plane before it. It is remarkable how long it takes before even many clever observers notice that one finger of the right hand (which is behind the left) is not grasping the left wrist, but instead pressing the stick against the invisible palm of the left hand so that the stick does not fall. When skilfully acted the phenomenal tension of the right hand and arm is so concentrated upon the left wrist that this convincing perceptual closure inhibits the arousal of antagonistic tendencies. Consequently, for many witnesses of this firm visual organization the notion never arises that a finger could have a different direction and the decisive and banal function which it actually possesses, despite the phenomenal arrangement. This signifies, however, that the arousal of ideas in such a situation is contingent upon the organization with which the situation itself is apprehended. In this instance an inappropriate organization was deliberately evoked. Conversely, he who would really understand the procedure must produce the required organization which could not be fully constructed in the *visible* situation. He misses a visible support for the stick and will therefore search for an invisible complement in the neighborhood of the stick. The idea of a definite finger position is the next thing to be reproduced which can bring to completion the adequate organization in the invisible area, and arises in those who are not permanently dominated by the phenomenal insertion of the right hand into a deceptive organization. Ac-

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[2] I am indebted to Professor Köhler for his interest in the study. The present paper is one of a series supported by the Social Science Research Council of New York.

> cording to this, the success of any sleight of hand hinges upon the goodness and convincingness of the false organization. If one simply placed the right hand from behind upon the left wrist one would not have much success with such attempts at 'magic.' "

SUMMARY

1. The hypothesis that understanding consists in the comprehension of structures was tested by means of ten unfinished activities which were presented in both "obvious" and "ambiguous" forms. Twenty subjects were required to indicate the direction and the intent of the actions.
2. According to the principal findings, the majority of instances are structurally conditioned, since the "obvious" situations were subjectively clearer and led to fewer errors.
3. It is conceivable that this material and technique may serve as a fruitful point of departure for experiments in related problems of social psychology.

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LES FACTEURS DE CONFIGURATION DANS LA COMPRÉHENSION DES ACTIONS

(Résumé)

Dans cette expérience vingt sujets ont regardé un individuel qui a fait les premières parties des activités ordinaires en dix situations extérieures différentes. Aucune des actions n'a été complétée, mais a été toujours interrompue avant les dernières étapes. On a demandé alors aux sujets d'indiquer ce que l'individuel a eu l'intention de faire. En cinq situations "claires," c'est-à-dire, des cas où les objets physiques s'adaptaient très naturellement au comportement précédent, les sujets ont deviné correctement un total de 81 pour cent; mais en cinq situations ambiguës, c'est-à-dire, des cas où les objets du milieu immédiat ont présenté des possibilités opposées l'une à l'autre, la proportion d'interprétations correctes a été réduite à 25 pour cent. On a employé les groupes de contrôle et les groupes expérimentaux en rotation pour rendre égales les différences dues aux variations dans l'intelligence

sociale, l'état mental, etc. La constitution objective du champ visuel exerce une influence puissante sur la compréhension de l'événement observé. On a reconnu ce fait dans les cas de camouflage où il s'agit des configurations statiques, mais l'évidence actuelle indique que c'est vrai aussi pour les suites temporelles et dynamiques. Au point de vue des phénomènes, si la structure de l'acte incomplet permet la "clôture," la signification du comportement est claire.

HARTMANN

GESTALTEINWIRKUNGEN BEI DEM VERSTEHEN VON HANDLUNGEN

(Referat)

In dieser Untersuchung beobachteten 20 Vpp. jemanden der die früher stattfindenden Teile von gewöhnlichen Handlungen bei 10 verschiedenen äusseren Gelegenheiten ausführte. Es wurde keine der Handlungen je vollendet. Immer wurden sie vor den letzten Stadien unterbrochen. Dann bat man die Vpp. anzudeuten, was der Ausführende hatte tun wollen. Bei 5 "einleuchtenden" Gelegenheiten—d. h., wo die konkreten Gegenstände der vorhergehenden Handlung angepasst waren—wurde in 81% der Fälle richtig geraten; aber in den 5 "zweideutigen" Fällen—d.h. wo die Gegenstände in der nahen Umgebung widerstreitende Möglichkeiten darbot—war das Prozent der Handlungen bei denen richtig geraten wurde auf 25 reduziert. Kontroll- und Versuchsgruppen wechselten ab, um die auf Verschiedenheiten in der gesellschaftlichen Intelligenz (social intelligence), geistigen Einstellung, u.s.w. zurückzuführenden Unterschiede gleichzusetzen. Die objektive Beschaffenheit des Gesichtsfeldes hat auf das Begreifen des beobachtenen Ereignisses eine starke Einwirkung. Diese Tatsache ist bei Fällen von Kamouflage in denen statische Gestalten in Anspruch genommen sind, erkannt worden. Die gegenwärtigen Befunde weisen aber darauf hin, dass sie auch für zeitliche und dynamische Serien gilt. Phänomenologisch gedeutet, wenn die Gestaltung der unvollendeten Handlung "Schliessung" (closure) vermöglicht ist die Bedeutung der Handlung klar.

HARTMANN

MENTAL AND ORGANIC EFFECTS OF READING*

From the Psychological Laboratory of the University of Chicago[1]

Ray Mars Simpson[2]

I. The Problem

The object of this experiment was to discover the distinctive mental and organic effects produced by reading detective stories in contrast with the reading of passages from academic material such as history, psychology, and geography.

To this end a study was made of the effects produced by the two types of material upon (1) breathing, (2) rate of pulse, (3) amount of blood pressure, (4) motor steadiness, (5) speed and accuracy of discriminating opposites, and (6) speed and accuracy in multiplication. In addition to the above data judgments were secured from the subjects as to the "exciting" and "non-exciting" character of the two materials.

In October, 1928, the feature editor of the Chicago Daily News came to the Psychological Laboratory of the University of Chicago seeking information concerning the effects produced by reading detective stories.

Shortly after this appeal for help a series of tests was given to a group of stenographers before and after a two-hour period devoted to reading "The Greene Murder Case" by S. S. Van Dine. The initial results indicated that two hours of reading decreased the rate of breathing, reduced the pulse rate, decreased the speed of reactions, increased the grip on a dynamometer, and interfered with the cancellation of certain letters in a newspaper column. These findings served merely to give a sense of direction to the more comprehensive study which follows.

II. Experimental Procedure

For stimulus-material, 17 short detective stories were selected from current issues of detective story magazines, as well as from

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[1] Special gratitude should be expressed to Professor Harvey Carr for incisive criticism and advice.

[2] Psychologist, Institute for Juvenile Research (Chicago).

famous detective classics written by noted authors, and were studied in contrast with passages from history, psychology, and geography. Fortunately, the choice of academic contrast-material was justified by the latter discovery that none of it was reported as exciting by the subjects.

In the main series of experiments 40 University students (24 women and 16 men), varying in ages from 18 to 41 years, were tested immediately before and immediately after each of six periods of reading on successive days. On the first, third, and fifth days the subjects read detective stories. On the second, fourth, and sixth days they read history, psychology, and geography. The time for each period of reading was kept constant at 15 minutes.

Detective stories were alternated in sequence with academic material from individual to individual.

Each subject was asked to approach the reading "just as if he were at home with 15 minutes to spare." A comfortable cushioned chair with desk-arm was provided for the reader. The room was well ventilated, well lighted, and quiet. Sampling covered every hour in the school day. Each subject came at the same hour on consecutive days.

Records were secured before and after each period of reading in order to measure the effects produced upon blood pressure, pulse rate, motor steadiness, naming opposites, and ability in multiplication.

It was deemed possible to obtain records of thoracic breathing during the period of reading without introducing any undue distraction. To secure such records a pneumograph was attached to the subject at the beginning of each period and six one-minute samples of thoracic breathing were recorded at equally spaced intervals during the period. The pneumograph was connected with a Marey tambour carrying a feather-weight celluloid pen. Direct records were made in ink upon a white sheet of paper rotating upon a kymograph drum. Time in seconds was recorded immediately below the record of respiration.

Perpendiculars were erected at the beginning and at the end of each one-minute sample in order to facilitate the measurement of rate and the length of the respiratory curve. The last cycle in each sample was divided into tenths in order to make accurate measurements of the incomplete cycles. The complete length of the respiratory curve, both inspiration and expiration slopes, was measured in sixteenths of an inch with a map measure. This method gave a clear-cut objective measurement of gross changes in breathing which avoided the numerous controversies involved in discussions of the significance of plateaus and inspiration-expiration ratios.

Blood pressure readings were taken on the left arm with a Bamanometer. The cuff of the instrument was attached loosely about the arm and allowed to ride in position until the second reading of pressure was taken following the period of reading. A stethoscope was used to detect the initial return of the pulse.

Pulse rate was taken at the wrist. A one-minute count was taken and checked by quarter consistency.

In the steadiness test the subject was required to insert a metal stylus into a series of small holes in a brass plate without touching the sides. Each contact was recorded as an error by an electric counter. Only the elbow of the arm was allowed to rest upon the table. A preliminary trial series of 27 holes was allowed each subject. The subject was required to follow the tick of a seconds-pendulum and to insert the stylus into 100 holes in fixed order at one-second intervals.

Four equivalent forms of the opposites test were taken from the Army Alpha Intelligence Examination and used in the work. Each test consisted of 32 pairs of words of the same or opposite meaning. One minute was allowed in which the subject checked as many pairs of words as possible as being the same or opposite in meaning. The number correctly checked (as well as the number of errors) was recorded.

In the multiplication test two-place numbers were multiplied by two-place numbers with the cipher eliminated. A record was kept of the time required to solve 10 problems. The number of errors was also recorded.

Following each period of reading the subjects were required to characterize the material as "exciting" or "non-exciting." The term "exciting" was not defined.

With the foregoing technique, the following results were secured.

III. General Results

A. Respiration. Three kymographic records of respiration were secured from each of 40 subjects while reading detective stories (a total of 120 periods). For purposes of comparison three similar records were taken from each of these same 40 people while reading passages from history, psychology, or geography. Six one-minute samples were recorded in succession at equally spaced intervals during each period of reading.

The average number of respiration cycles for each of 6 one-minute samples, covering an interval of 15 minutes, is recorded in Table 1. The average length of the respiratory curve, taken during one-minute intervals at the beginning and at the end of 15-minute periods of reading for both kinds of material, is presented in Table 2. The significance of these tables is revealed more clearly in Figures 1 and 2.

It should be borne in mind that, immediately following the test period, each subject was asked to answer the following question: "Do you consider the passage just read as exciting or non-exciting?" The replies to this question indicate that 30% (36 periods) of the

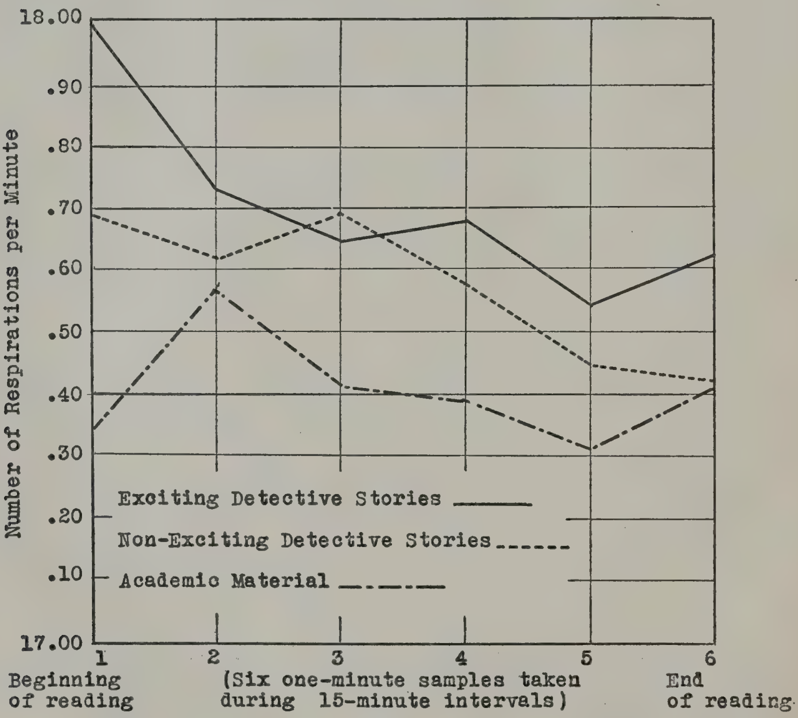

FIGURE 1
RATE OF BREATHING

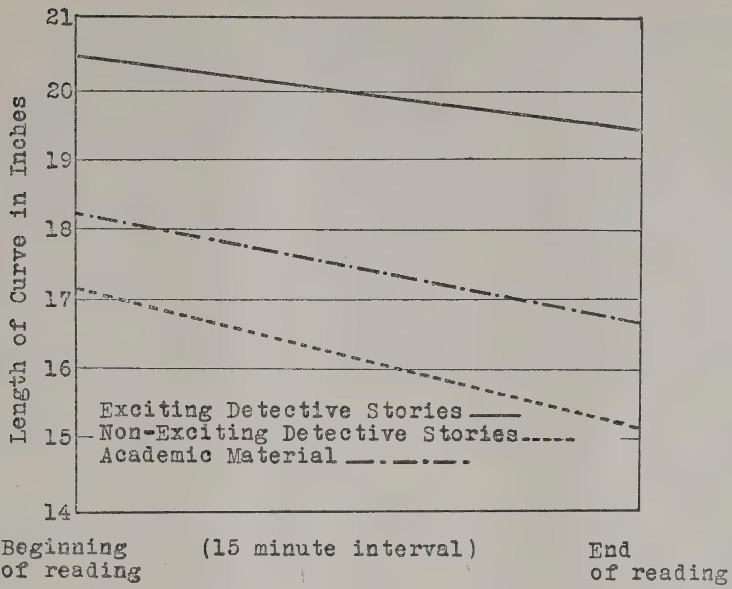

FIGURE 2

Average Length of Respiratory Curve Measured in Inches

detective stories were classified as exciting. These 36 periods are considered together in the accompanying tables and charts. The remaining 70% (84 periods) of the detective stories were considered to be "non-exciting." The academic material was also characterized as "non-exciting" by every subject.

Figure 2 shows that the average length of the respiratory curve

TABLE 1

Average Number of Respiration Cycles per Minute

	Six one-minute intervals					
	1	2	3	4	5	6
a. Detective Stories						
36 periods of reading reported "exciting"	17.99	17.72	17.65	17.68	17.54	17.61
84 periods of reading reported "non-exciting"	17.69	17.62	17.68	17.58	17.45	17.42
b. History, Psychology, and Geography						
120 periods of reading	17.35	17.57	17.41	17.39	17.31	17.41

TABLE 2

AVERAGE LENGTH OF RESPIRATORY CURVE

	Beginning of reading	End of reading
a. Detective Stories		
36 periods of reading reported "exciting"	20.48 inches	19.43 inches
84 periods of reading reported "non-exciting"	17.11 inches	15.10 inches
b. History, Psychology, and Geography		
120 periods of reading	18.13 inches	17.61 inches

was consistently longer during the reading of "exciting" detective stories in contrast with the "non-exciting" detective stories and academic material.

The method used in measuring respiration curves deliberately ignores all transitory changes and considers only the gross general trend in both the rate and the length of the respiratory curve. Since the method of handling the respiration curve gives a combined figure for rate and amplitude, it is possible that these figures might be used as an index of the decrease in amount of work done by the lungs during a 15-minute interval of reading.

At this point it might be well to state that the pulse rate and the length of the respiratory curve both decreased simultaneously

TABLE 3

"EXCITING" COMPARED WITH "NON-EXCITING" PERIODS (INTROSPECTIVE REPORTS)

		"Exciting" detective stories		"Non-exciting" detective stories		Academic material	
		No.	%	No.	%	No.	%
Rate of breathing	decrease	23	64	49	58	65	54
	no change	3	8	5	6	4	3
	increase	10	28	30	36	51	43
Length of curve	decrease	25	69	64	76	86	72
	no change	—	—	3	3	—	—
	increase	11	31	17	21	34	28
Number of periods		36		84		120	

in 55% of the cases covering detective stories in which the length of the respiratory curve decreased (i.e., in 89 of the 120 periods).

In Table 3 a summary of the introspective reports is presented with the accompanying changes in respiration. By combining the results for detective stories in Table 3 a progressive decrease in the rate of breathing is shown in 60% of the periods devoted to detective stories and in 54% of the periods spent in reading academic material; furthermore, the length of the respiratory curve decreases in 74% of the periods spent in reading detective stories and also decreases in 72% of the periods devoted to academic material. In general, it can be stated that there is very little difference in the effects produced by reading detective stories or academic material when stated in terms of the percentages of periods showing changes in respiration.

In order to check the reliability or consistency of the results, the data was divided by random selection into several different groups. Group comparisons were made. Reported "exciting" detective stories, in contrast with all other groupings reported as "non-exciting," were accompanied by a longer respiratory curve in every comparison. Furthermore, the reported "exciting" material was generally accompanied by a faster rate of breathing.

No attempt was made to define "exciting" except in terms of the changes in respiration which accompanied the reports of "exciting." Figures 1 and 2 show that reported "exciting" material, in contrast with "non-exciting" material, is generally accompanied by a higher rate of breathing. Furthermore, the reported "exciting" material was always accompanied by a longer respiratory curve in every group comparison.

The following factual statements on respiration may be derived from the data covering 120 periods devoted to each type of reading material:

1. Relative to the rate of breathing:

a. The average rate of breathing was consistently faster throughout the periods devoted to detective stories.

b. The average rate of breathing decreased during the reading of detective stories and remained fairly constant during the reading of academic material.

c. The *final rate-level* for breathing was about 2/10 of one cycle higher for the "exciting" material in contrast with "non-exciting" material.

2. Relative to the average length of the respiratory curve:

a. The average length of the respiratory curve was consistently longer during the reading of "exciting" material in contrast with "non-exciting" material.

b. The average length of the respiratory curve at the *final level* was 2.72 inches longer for the "exciting" detective stories, than for the academic material; and was slightly over 4 inches longer than the curve for the "non-exciting" detective stories.

3. The length of the respiratory curve and the rate of breathing decreased simultaneously in 50.0 per cent of the detective stories and in 41.0 per cent of the academic material.

B. Pulse Rate. Records dealing with pulse rate were secured immediately before and after each of one hundred 15-minute periods devoted to each type of reading material. Detective stories were thrown into contrast with history and geography. The averages for both types of material are presented in Table 4.

Table 4 shows that the *final level* of pulse rate is consistently lower following periods devoted to reading detective stories. Detective stories were accompanied by a total average drop of 2.26 ±0.202 beats per minute, while a total increase of 0.49±0.184 beats per minute was shown for the academic material. The initial level for detective stories was higher in two of the odd-even comparisons and lower in the other two. The figures indicate that detective stories operate to reduce the pulse rate to a lower level of performance than that shown to accompany the reading of history and psychology.

In Table 5 the percentage of periods which were accompanied by a decrease, an increase, or no change in pulse rate is shown.

TABLE 4

Average Pulse Rate per Minute

		Initial level	Final level
Detective stories	Odd—50 periods	80.00	78.40
	Even—50 periods	79.48	76.56
	Total—100 periods	79.74	77.48
Academic material	Odd—50 periods	78.24	79.76
	Even—50 periods	80.26	79.72
	Total—100 periods	79.25	79.74

In Table 6 the reported "exciting" detective stories are thrown into contrast with "non-exciting" material. It is evident that the percentage of cases showing an increase in pulse rate was greater for both groups of "non-exciting" material.

In conclusion, it seems evident that detective stories tend to reduce the pulse rate more than academic material.

C. Blood Pressure. Detective stories were thrown into contrast with geography in the study of blood pressure. Fifty periods of 15 minutes each are considered in Tables 7 and 8. Table 7 shows that the *final level* of blood pressure had fallen to practically the same level for both types of material. It should also be noted that the initial level for detective stories was higher than that for geography.

The percentage of periods showing a decrease, an increase, or no change in blood pressure is presented in Table 8.

A comparison of men with women revealed practically no difference between the two groups.

TABLE 5

PERCENTAGE OF 100 PERIODS SHOWING INCREASE, DECREASE, OR NO CHANGE IN PULSE RATE

	Detective stories	Academic material
Decrease	54%	36%
No change	22	20
Increase	24	44

TABLE 6

PULSE RATE—"EXCITING" COMPARED WITH "NON-EXCITING" PERIODS

	Decrease	No change	Increase
"Exciting" detective stories	47.62%	38.10%	14.28%
"Non-exciting" detective stories	47.73	15.90	36.37
"Non-exciting" academic material	36.00	20.00	44.00

TABLE 7

BLOOD PRESSURE

(In millimeters of mercury)

	Before	After
Detective stories	115.64	109.30
Geography	114.66	109.08

TABLE 8

PERCENTAGE OF 50 PERIODS SHOWING INCREASE, DECREASE, OR NO CHANGE IN BLOOD PRESSURE

	Detective stories	Academic material
Decrease	90%	88%
No change	4	8
Increase	6	4

TABLE 9

BLOOD PRESSURE—"EXCITING" COMPARED WITH "NON-EXCITING" PERIODS

	Decrease	No change	Increase
"Exciting" detective stories	93.75%	6.25%	—
"Non-exciting" detective stories	90.48	4.76	4.76%
"Non-exciting" academic material	88.00	8.00	4.00

TABLE 10

STEADINESS—AVERAGE NUMBER OF ERRORS IN 100 TRIALS

		Initial level	Final level
Detective stories	Odd—25 periods	12.48	7.40
	Even—25 periods	13.92	8.24
	Total—50 periods	13.20	7.82
History	Odd—25 periods	9.68	7.44
	Even—25 periods	10.28	8.96
	Total—50 periods	9.98	8.20

Table 9 shows that there is only a slight difference between the changes in blood pressure for "exciting" and "non-exciting" material. The blood pressure drops in about 90% of all cases.

In conclusion, it seems to be very plain that the blood pressure falls to practically the same *final level* at the end of a 15-minute period devoted to either detective stories or geography.

D. Steadiness. The results on steadiness were secured during 100 trials at the steadiness tester immediately before and after reading passages from detective stories and history. The averages are given in Table 10. A comparison of the totals shows that more errors accompanied the detective stories before reading while fewer errors accompanied the detective stories after reading. In order to ferret out whether this difference was significant the results were dichotomized into two equal parts (odd- and even-numbered items) and cross comparisons were made.

The odd-even comparisons shown in Table 10 reveal the fact that the *initial* level for detective stories is marked by more errors at the steadiness tester. It is possible that some unforseen factor of anticipation may account for this. In spite of the higher initial level of errors the *final level* for detective stories is marked by fewer errors for the total average and for the averages for the subgroup in three out of four comparisons.

In conclusion, it is evident that the steadiness of individuals tends to be better following the reading of detective stories than following the reading of history. It is significant that there were more errors immediately before the reading of detective stories.

E. Ability to Discriminate Opposites and Ability in Multiplication. A careful analysis of the data collected from 75 subjects shows that neither type of material exercises any certain effect upon speed and accuracy in the ability to discriminate between opposites or upon the ability to solve simple problems in multiplication. One-minute tests were applied immediately before and after reading. The final records did not consistently differ from the initial records, neither did the records for one type of reading material differ from the records for the other.

IV. Summary

1. Relative to the average rate of breathing:

a. The average rate of breathing was consistently faster throughout the periods devoted to detective stories.

b. The average rate of breathing decreased during the reading of detective stories and remained fairly constant during the reading of academic material.

c. The *final rate-level* for breathing was about 2/10 of one cycle higher for the "exciting" material in contrast with the "non-exciting" material.

2. Relative to the average length of the respiratory curve:

a. The average length of the respiratory curve was consistently longer during the reading of "exciting" material in contrast with "non-exciting" material.

b. The average length of the respiratory curve at the *final* *level* was 2.72 inches longer for the "exciting" detective stories than for the academic material; it was slightly over 4 inches longer than the curve for the "non-exciting" detective stories.

3. Reading detective stories tends to reduce the *pulse rate* more than reading academic material.
4. The *blood pressure* falls to practically the same final level at the end of 15-minute periods devoted to either detective stories or geography.
5. Motor *steadiness* is improved more by readnig detective stories than by reading history.
6. No consistent difference was discovered in the influence exerted by reading either detective stories or academic material upon the speed and accuracy in discriminating between opposites or in solving simple problems in multiplication.

LES EFFETS MENTAUX ET ORGANIQUES DE LA LECTURE

(Résumé)

On a employé quarante étudiants comme sujets pendant 120 périodes de lecture. On a fait subir des tests avant et après des intervalles de lecture de 15 minutes. On a trouvé (1) que la vitesse moyenne de la respiration a été constamment plus rapide pendant des périodes consacrées aux romans policiers en contraste avec la lecture scolaire. La vitesse moyenne de la respiration a diminué pendant la lecture des romans policiers et est restée assez constante pendant la lecture scolaire. Le niveau final de vitesse de respiration a été plus élevé pour la lecture rapportée comme "émouvante" en contraste avec la lecture "non émouvante." (2) La longueur moyenne de la courbe respiratoire a été constamment plus longue pendant la lecture rapportée comme "émouvante" en contraste avec la lecture "non émouvante." La longueur moyenne de la courbe respiratoire au niveau final a été plus longue pour les romans policiers "émouvants" que pour la lecture scolaire ; elle a été un peu plus longue que la courbe pour les romans policiers "non émouvants." (3) La lecture des romans policiers a tendu à réduire la vitesse du pouls plus que la lecture scolaire. (4) La pression sanguine est tombée à presque le même niveau final au bout des périodes de 15 minutes consacrées ou aux romans policiers ou à la géographie. (5) La fermeté motrice est devenue meilleure quand on a lu des romans policiers que quand on a lu l'histoire. (6) On n'a trouvé nulle différence constante dans l'influence exercée par la lecture des romans policiers et celle de la matière scolaire sur la vitesse et la précision de la discrimination des contraires ou de la résolution des problèmes simples de multiplication.

SIMPSON

DIE GEISTIGEN UND ORGANISCHEN EINWIRKUNGEN DES LESENS

(Referat)

Es dienten 40 Studenten wärend 120 Leseperioden als Versuchpersonen. Es wurden Prüfungen vor und nach 15-minutenlangen Leseperioden gemacht. Man fand (1) dass die durchschnittliche Schnelligkeit des Atmens durch Perioden hindurch, die dem Lesen von Detektivromanen gewidmet wurden grösser war, als beim Lesen von akademischen Material. Die durchschnittliche Schnelligkeit des Atmens nahm während des Lesens von Detektivromanen ab, und blieb während des Lesens von akademischem Stoff ziemlich konstant. Die *endgültige Schnelligkeit* des Atmens war bei einem Stoff, der als "aufregend" gemeldet wurde, grösser, als bei "nicht-aufregendem" Stoff. (2) Die durchschnittliche Länge der Atmungskurve war während der Lektüre von sogennantem "aufregendem" Stoff grösser, als bei "nicht-aufregendem" Stoff. Die durchschnittliche Länge der Atmungskurve war auf dem endgültigen Niveau bei der Lektüre von "aufregenden" Detektivromanen grösser, als bei akademischem Stoff; sie war auch etwas länger, als bei "nicht-aufregenden" Detektivromanen. (3) Das Lesen von Detektivromanen neigte mehr als das Lesen von akademischem Stoff dazu, die Schnelligkeit des *Pulsschlags* herabzusetzen. (4) Der *Blutdruck* fiel nach 15-minutenlangen Leseperioden zu fast dem selben endgültigen Niveau, ob des Lesematerial aus Detektivromanen oder aus Geographie bestand. (5) Die *motorische Festigkeit* (steadiness) wurde durch das Lesen von Detektivromanen mehr verbessert, als durch des Lesen von Geschichte. (6) Weder das Lesen von Detektivromanen noch das Lesen von akademischem Stoff schien auf Schnelligkeit und Genauigkeit der Unterscheidung zwischen Gegenteilen (opposites) oder der Lösung einfacher Multiplikationsaufgaben irgend eine definitive unterscheidende Einwirkung zu haben.

SIMPSON

SHORT ARTICLES AND NOTES

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A METHOD OF GREATLY INCREASING SENSITIVITY TO COLOR CONTRAST[1]

C. E. FERREE AND GERTRUDE RAND

The method and phenomenon to be described have been demonstrated and used by us both in lecture and laboratory teaching for many years. The method has proven of great value in many cases in greatly increasing contrast where it is faint, in bringing latent contrast above the threshold, and in showing that contrast exists in situations where it had not previously been known to exist. The fact of the increased sensitivity also should be of interest in relation to theory.

We have noted in previous papers that both brightness and color contrast are greater at low than at high illuminations. Two possible factors in this difference in case of color contrast are (*a*) greater physiological induction at low than at high illumination, and (*b*) the greater effectiveness of a given amount of induction for sensation at low than at high illumination. The second factor pertains to the inhibitive action of the achromatic on the chromatic excitation. That is, the greater is the achromatic excitation, the more is the color excitation inhibited. Whether this well-known effect is sufficient in itself to explain the difference in the amount of contrast experienced at high than at low illumination we are at present not prepared to say.

The fact which we wish to bring out in this paper, however, is the very great increase in contrast that is experienced while the illumination is decreasing. The amount of contrast that can be brought out while the illumination is decreasing when the optimum rate of decrease is used is almost unbelievable to one who has not observed the phenomenon. Also, as we have stated above, by the use of this type of technique we have been able in many cases to make latent contrast manifest and to demonstrate in at least two situations important to theory that contrast is present where contrast had not before been known to exist. Some of the more important of these cases will be discussed in later papers.

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The selection of conditions for the use of the method is somewhat important. Factors which should receive attention are the quality or color of the illumination, the range of change of the illumination, and its rate of change. It is very important that the illumination should have daylight color; the range should not be less than from 10-15 foot-candles to darkness and the rate should be comparatively fast, i.e., the entire change should not take more than 1 second.

Just what are favorable conditions can perhaps be made clear by the description of one of our laboratory experiments. The experiment was conducted in a room 15 feet long, 8 feet wide, and 10 feet high. The walls were of rough, unfinished plaster. At one end of the room was a window 2 feet wide and 3 feet high. This window was provided with a heavy light-proofed door or shutter opening inwards. The bottom of the window was about 5.5 feet from the floor. For the purpose of this experiment the door, which could be closed quickly and at a uniform rate, was much better than any light-proofed curtain that could have been used. On a table in front of this window were mounted two rotary color mixers, facing the window so as to receive its full illumination. On one of these mixers were mounted two discs of the same color, one with a radius of 11 cm. and the other of 5 cm.; and one black and one white disc with a radius of 5.8 cm. The black and white discs were adjusted to give a gray of the brightness of the color. When rotated, the discs presented two circular colored surfaces with radii of 11 and 5 cm., and a ring between them 8 mm. broad, of gray of the brightness of the color. With the greater number of observers the effect was the most vivid when red was used as the inducing color. The following proportions of white and black of the Hering papers were used with good success with the standard red of that series of papers: white 85 degrees, black 275 degrees. While, as stated, the most vivid effect was obtained with red, a very strong effect was obtained with all colors. On the second disc were mounted a disc of the contrast color and a gray disc of the brightness of the color. The proportions of these discs were varied to match the saturation of the contrast color induced over the gray ring on the first disc. The experiment consisted of three parts: (*a*) the contrast was obtained and measured at the selected high illumination with the eye adapted to that illumination; (*b*) the contrast was obtained and measured at a low illumination (0.25-0.50 foot-candle) with the eye both adapted and not adapted to that illumination; and (*c*) it was observed while the illumination was changing. At the maximum point of saturation obtained during the change, the richness and brilliancy of the contrast color could not be represented in any pigment color. With a little practice the optimum rate of closing the door could be easily attained.

Two interesting variations of the experiment may also be described. (1) In producing the change of the illumination, if one carries the decrease far enough, a point is reached at which the inducing color becomes colorless. At this point the contrast color, however, can be sensed when red and yellow are used as inducing colors. This affords but one of several illustrations of the fact that contrast can be sensed when the inducing color is below the threshold of sensation. When the inducing color is red, for example, and the induced color is green, red darkens and loses its saturation rapidly with decrease of illumination; green, on the other hand, the induced color, lightens but not enough to interfere seriously with its saturation. Thus, under the influence of the achromatic change or excitation, the red as color becomes sub-threshold at a point at which green can still be sensed. The interesting fact here is, of course, that the red stimulus, after it has lost its power to arouse color in the positive sensation, can induce an excitation which is sensed as color. Similarly, when the inducing color is yellow and the induced color is blue, the yellow darkens with decrease of illumination, and blue, the induced color, lightens, but also not enough to interfere seriously with its saturation. Again, under the influence of the achromatic change or excitation, the inducing color becomes sub-threshold at a point in the decrease of illumination at which the induced color is still supra-threshold.

These may be considered extensions of Purkinje's observations. Purkinje observed that with change of intensity of stimulating light the colors undergo selectively changes in brightness, hue, and saturation, not in brightness alone as is often ascribed to his observations. Purkinje, however, did not point out the action of the achromatic upon the chromatic component of the color excitation, nor did he extend his observations to the accompanying contrast phenomena. The action of the achromatic upon the chromatic excitation may be observed in positive, after-image, and contrast sensations, and in all the phenomena due to the combining or mixing of positive excitations with after-image and contrast excitations. The Purkinje phenomena comprise only certain aspects of the effect as pertaining to positive sensation.

(2) It is probably well known that when one stares at a colored surface for some seconds (40-60), until adaptation has been strongly set up, and then quickly decreases the illumination to some lower level, e.g., one-half or less than one-half the original illumination, the color will be seen in its complementary hue. That is, at the reduced illumination it is seen in the color of the after-image. When the illumination is made static at this point and the observation is continued, the complementary color gradually be-

comes less saturated and the surface finally appears in the original hue. When the experiment is conducted with the contrast discs described above and the illumination is decreased at a rate suitable for making the observation, near the time of change of the inducing color to the complementary hue the gray ring takes the hue of the after-image of the contrast color, i.e., the original hue of the inducing color. The statement "near the time of change of hue in the inducing color" is made because when the inducing color is red, yellow, or orange, the change comes in the contrast color before it takes place in the inducing color. On the other hand, when the inducing color is blue, green, or blue-green, the change in hue in the inducing color takes place before the change in the contrast color. In case of the former group of colors the hue complementary to the after-image of the inducing color is already present, for example, at the neutral stage in the change of the inducing color from positive to complementary or what is sometimes called negative. That is, again we have the presence of the contrast color when no color is sensed in the inducing surface. When the inducing surface is blue, green, or blue-green, the change in hue does not come until after the change in the hue of the inducing surface has taken place.

When the illumination is made static at some point after the hue changes have taken place and the surface of the disc is observed for a period, the following phenomena are seen to take place. The complementary hues both in the inducing and the contrast surfaces gradually fade out and finally change back to the original inducing and contrast hues. In this process the color in the contrast ring is seen to fluctuate. In the earlier stages of the fluctuation it changes alternately from the same hue as the original inducing color to the complement of this hue, and in the later stages it becomes alternately neutral gray and the hue of the original inducing color. The changes can be more easily understood perhaps by selecting a concrete instance. When the hue of the inducing color was red, and the contrast ring green, the inducing color became green at a certain point in the decrease of the illumination, and the contrast ring became red. In the earlier stage of the fluctuation of the contrast ring it alternated between red and green of the brightness of the green in the inducing surface. In changing from red to green, the green from the surrounding surface appeared to spread over the contrast ring, obliterating the red and giving a uniform green hue to the entire surface of the disc. Later, the alternation was between red and a green lighter and less saturated than the green of the inducing surface; still later the alternation was between red and gray; and, finally, the inducing surface changed back to the original red and the contrast ring became a comparatively unsaturated green.

In connection with these observations it becomes an interesting point for discussion whether the change of hue of the green of the contrast ring to red at or near the point of change of the inducing color from red to green is a phenomenon of after-image of the previous contrast, or contrast induced by the green after-image of the original inducing color. On this point it may be said that, in fluctuating, this ring behaves as an independent after-image. It goes through its changes independent of any connection, at least any close connection, with changes in the inducing surface. In fact, the color in the inducing surface was not observed to fluctuate at all. In brief, three reasons may be noted for believing the phenomenon to be an after-image of contrast rather than contrast induced by the after-image. (*a*) As already stated, in fluctuating the color in the contrast ring behaves as we would expect were it an independent after-image. (*b*) For the part of the retina concerned, the contrast reaction is initial or primary. It does not seem at all unreasonable that a second or after-image reaction should occur in this part of the retina. On the other hand, the after-image of the inducing color is itself a second reaction in its part of the retina. To expect it to induce contrast is to grant the possibility of a tertiary reaction in that part of the retina, i.e., if contrast is to be considered as a spreading or induction of the after-image excitation, which is by no means unthinkable, to grant that an after-image can induce contrast is in effect to grant the possibility of an after-image arousing an after-image. This scarcely seems probable. And (*c*) in a previous paper (2) we presented results from three types of experiment to show that the phenomenon which we have called the Purkinje-Brücke phenomenon is an after-image of contrast rather than contrast induced by an after-image. The Purkinje-Brücke phenomenon, so far as the problem of explanation is concerned, is essentially very closely similar to the phenomenon under discussion here.

If the phenomenon is to be considered an after-image of contrast a question may be raised as to why the change to the after-image should come with decrease of illumination and at a time nearly coincident with the change of the inducing color to the after-image stage. That the change in the inducing color itself should take place under the conditions noted is, perhaps, not difficult to understand. The positive sensation has been growing weaker due to the development of the negative excitation through a period of 40-60 seconds under a high intensity of light. As long as this intensity of illumination is maintained, the effect of the increasing strength of the negative process, be it inhibitive in nature or merely a condition of fatigue, will be only a loss in saturation. But, if the illumination is suddenly decreased to a lower level, the negative process built up at the higher level of illumination is stronger than the positive process at the lower illumination, and therefore determines or gives the hue to the resultant sensation. For the contrast sensation, however, the case might seem different. That is, the contrast sensation is not directly aroused by the stimulating light. It is aroused only through the effect of this light on the inducing color, and therefore can be affected by a change in the stimulating light only through an effect of this change on the inducing excitation. The effect of the change of illumination is, so to speak, to withdraw or inhibit the original inducing excitation and to substitute for it the negative excitation. This withdrawal of the original inducing excitation gives an opportunity for the contrast excitation to pass over into the after-image stage.

In conclusion, a curious fact may be noted. The after-image seems never to be projected quite to the plane of the surface viewed. When that surface is a color, the after-image seems to be superimposed upon it as a veil or overlay which moves off the surface when the eye moves and back upon it again when the eye retakes its former fixation. If the after-image is blue, for example, and of sufficient saturation and the surface viewed is yellow, when the after-image is superimposed the surface appears gray, i.e., the blue excitation is completely neutralized for sensation. Yet when the eye is moved so that the after-image area no longer is superimposed on the yellow surface the blue after-image is seen unimpaired as to saturation. That is, the previous neutralization by the yellow excitation has not affected it to any detectable degree. If the color process is photochemical this would seem to mean that it cannot be the actual decomposition of any visual substance that arouses the color sensation, else the decomposition of the substance which gave rise to the sensation of blue in the above case must have been completely checked by the yellow excitation. If this had been true it seems hardly probable that the blue after-image could have been seen as strongly as before when the eye was moved from the position of superimposition. It must be rather that the sensation is conditioned by some effect or product of the decomposition and it is these effects or products which neutralize each other. In other words, the primary receptor action, at least in its negative stage, is not affected in the process of neutralization. This conception would be quite in accord with a phenomenon which one of the writers described many years ago, viz., the streaming phenomenon (1). In this streaming, color was seen to be carried from one field across another as if some substance capable of exciting sensation was being transported from a given area of the retina to a neighboring area.

In the foregoing comments, the after-image is regarded as being due to an after-excitation or process of a suitably specialized nature in the retina. That a colored after-image can be caused by an unbalancing of the response of the retinal fibers by fatigue seems to be rendered improbable by the fact that the color is very much more saturated when the projection is on a black than on a white background or in a dark than in a light field of view. That is, maximum color is obtained under conditions of projection that should in terms of the theory give no color at all or, at least, a minimum of color. So far as colored after-images are concerned, the Fechner-Helmholtz explanation of after-images does not seem to merit serious consideration and has not, therefore, been taken into account in the above conjectures, extremely tentative even though they are intended to be.

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THE POSITIVE AFTER-IMAGE OF TWILIGHT VISION[1]

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In 1916 Swindle (6) reported an hitherto unnoted positive after-image that apparently depends upon a high degree of dark-adaptation prior to the stimulation. This after-image reproduces exactly the color of the stimulus object, lasts for an especially long time, and may recur after a long interval either spontaneously or as the result of a weak illumination.

In an attempt to observe the "positive after-image of long duration," we came upon a variation of conditions that gave an after-image distinctly different from that reported by Swindle. It depends also upon dark-adaptation and upon the brief exposure of a stimulus of fairly high intensity, but it does not reproduce the color of the stimulus object.

Procedure I

O was seated inside a dark chamber in a dark room. At a distance of 1.5 meters in front of him was a screen of black velvet on which were arranged rectangles of red, yellow, green, and blue 2 cm. by 6 cm. in the form of a square with a 2-cm. disc (visual angle 47') of white in the middle. The internal diameter of the square was 8 cm. (visual angle 3°3'). In the center of the white disc was a hole 1 mm. in diameter illuminated by a small light behind the screen, which served as a fixation point. Below and in front of the stimulus screen was a light box furnished with two 500-watt lantern bulbs so screened that when a shutter was opened the stimulus was evenly and brilliantly illuminated but no light shone directly into O's eyes. Before entering the dark chamber O read the following instructions:

> "At the ready signal you are to fixate the point of light in front of you. Shortly after 'Now' a bright light will be thrown

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[1] A preliminary report of the results obtained in Procedure I was made before the American Psychological Association at the 1925 meeting.

> upon the screen for a short time. You are to maintain your fixation as steadily as possible during and after exposure. Report 'Now' when any after-effect appears, 'Gone' when it disappears, and 'Change' if it alters in any way. At the end you will be asked to describe the after-effects."

When O had been in the dark chamber for 5 minutes, the first exposure was made. Two minutes were allowed to elapse before the next observation was taken, and so on. The optimal exposure time was found to be 1 second. Careful descriptions and quantitative reports were obtained from four O's, two of whom had had considerable experience in experimental observation, and two of whom were previously untrained. At least one set of observations was taken from each of 25 untrained O's.

Color of Image. All O's reported after-images that were essentially the same. The color of the after-image was a grayish-white with a tinge of blue variously described as a "gray-white," "a white light with a hint of blue," "moonlight-white," "an almost phosphorescent color," "a ghostly white," and, with one exception, was independent of the color of the stimulus. The parts of the stimulus that were green, blue, and yellow all gave the above image, but for the red stimulus it did not appear at all. Under our conditions one O failed to get any after-effect from the red stimulus; one reported a relatively brief positive—a dull brownish red; another got only a dark green; and the fourth saw a very short bright positive followed by the negative. These after-images from the red were simultaneous with the first part of the "bluish-gray-white" image from the other colors and disappeared before the end of it.

Position on the Retina. The spatial arrangement of the parts of the stimulus was such that they fell well outside the fovea with the exception of the white fixation disc which was small enough to lie entirely within the fovea. This white disc never for any O appeared in the after-image though all of the conditions other than its position were especially favorable for its recurrence. The size of the stimulus square was varied and when it was made small enough so that it encroached upon the boundaries of the foveal area the "bluish-gray-white" after-image became poorer. No stimulus that fell entirely within the fovea appeared in the after-image. With a foveal stimulus and a longer exposure (2 sec.) the after-image consisted of a positive followed by a negative after-image.

Duration and Temporal Course. The duration of the after-image remained relatively constant for individuals and did not vary greatly from one O to another. The shortest average time for an O was 10.4±1.4 seconds; and the longest average time was 13.8±1.5 seconds. The appearance and disappearance of the image followed a definite and constant temporal course of 3 stages. Following the cessation of the stimulus there was a period of 1 to 1.5 seconds before the image was clearly perceived. This period was occupied first by a "flare" of light, then by "darkness" out of which the image emerged. When it first appeared, the image, for one O, was a true-colored positive of all four rectangles. In this case the positives were very quickly replaced by a green negative of the red stimulus and by the bluish-gray-white images of the other three rectangles. The remaining O's reported no such positive image or stage. The "bluish-gray" image of three rectangles formed quickly upon the background of "darkness" and stood out for several seconds all of one color, very vivid, and frequently perceived as objective and set off from the background. One O said: "It did not look like an after-image; I seemed rather to be seeing real objects, all of a ghostly hue." After this the image faded, first by losing its distinctness of outline and then by spreading out in all directions, at the same time losing in intensity until it became a large patch that finally merged into the dark gray of the background. Individual variations in the duration of the image may be attributed to the difficulty and the difference among O's in deciding when the image was gone, i.e., whether the report should be made when the distinct rectangles could not be seen or when all of the gray patch had disappeared.

Perceptual Characteristics. The perceptual characteristics of the image brought out by our first procedure are unlike those of the usual positive and negative after-image. The latter have been shown to appear as color film rather than as surface or bulk (1, 3). Negative after-images obtained from surface or solid objects rarely appear as sharply outlined as the stimulus object. They consist of patches of color with hazy outlines which overlie the background or other objects and which change in size with fixation. In contrast, our "blue-white" image in its optimal stage retains with startling reality the perceptual characteristics of the stimulus. Under very simple and crude conditions with a naïve group, this aspect is evidenced by the exclamations of the O's. In this respect, the image resembles Jaensch's "eidetic image" (2).

PROCEDURE II

In our second experimental procedure, we endeavored to simplify the perceptual conditions under which the O's observed and to obtain more precise quantitative values for the duration and the intensity of the after-image. We used as a stimulus a spot of light 25 mm. in diameter projected upon the back of a ground glass screen 150 cm. from the O. This spot was given sharp definition by an opaque screen behind the ground glass, with a 25-mm. opening. The light was furnished by a 500-watt concentrated filament bulb and was directed upon the screen by means of a projection lens. The duration of the stimulus exposure was controlled by a revolving shutter actuated by gravity. On each side of and 1 cm. above the stimulus at a distance of 15 cm. was a small fixation light of an intensity that was just visible. The O's head-rest was so constructed that he could see with only one eye, and the position, right or left, of the fixation point corresponded with the eye in use for the particular observation. The position of the stimulated spot on the retina thus lay on the temporal side and a little above the fovea.

By means of signal keys, records were made on a multiple recording device of the instants of beginning and of cessation of the stimulus, of beginning and of change in the after-images, and of the temporal course of the intensity of the image. The instructions to the O's were identical with those of Procedure I.

Results. The quality of the image brought out by this procedure was the same as that with the illuminated objects, namely, a "bluish-white-gray." The temporal course of the series of after-images was also the same. The cessation of the stimulus was followed by a bright "flash" or "glare" of light that was not easily marked off from the stimulus itself. Next came a dark interval and then the "blue-white-gray" image appeared. Sometimes a negative image of low intensity followed. The image came to its maximum intensity very quickly (1.3 seconds), and faded out gradually by decreasing in intensity and losing its definite outline. On a scale of five intensities above zero, in which 5 represented the brilliance of the stimulus, the average intensity for all O's was 3.8±.6.

Table 1 shows the average duration of the "blue-white-gray" after-image for every O. Those with the white stimulus are only slightly longer for this procedure than for the first, in spite of the greater stimulus intensity and the better conditions of observation of the final stages. The duration of the more brilliant period of the image is very nearly the same in both cases. The colored stimuli were produced by inserting colored glass between the light and the screen. This necessarily reduced the total intensity of the illumination. Consequently, the duration of the image was shorter and its intensity was less (3.1±.4). However, its relation to the color of the stimulus as shown in Procedure I was still maintained.

TABLE 1

Duration of the "Scotopic After-Image" in Seconds

Stim. color → Observer	White	Blue	Green
C	10.0±2.4	8.6±2.3	7.6±1.6
H	9.3±3.0	6.1± .8	5.5±1.6
Mc	17.0±4.7	10.4±2.4	6.9±1.5
P	5.7±2.0		

At first, observations were begun as soon as O entered the dark chamber so that we could check the effect of dark-adaptation. During the first 5 minutes only an occasional very brief and indefinite "blue-white-gray" image was obtained. Even these were obscured by stronger true positive and negative effects. Only after 10 minutes' dark-adaptation did the image become sufficiently stable to justify an attempt to make records. Increasing dark-adaptation beyond this time produced no measurable change in the duration or intensity of the image.

We may summarize as follows the facts brought out by our two procedures. The after-image that we have been investigating

1. occurs only under partial dark-adaptation and best under complete dark-adaptation;
2. is always of a bluish-gray-white color;
3. is absent when the stimulus is red;
4. is absent when the stimulus falls upon the fovea;
5. runs a temporal course of fairly constant length;
6. grows and fades in a manner unlike the ordinary positive or negative after-image;
7. reflects the perceptual aspects of the stimulus more than is usual with after-images.

These characteristics indicate that the after-image we have described is a matter of twilight vision and is to be understood in terms of the functioning of the rods. As a possible explanation, we may suppose that we are concerned here with the photo-chemical action of the "visual purple." Under the prior dark-adaptation this substance has been generated in the extra-foveal region. The short, intense stimulation has produced a bleaching of the visual purple over the area stimulated except where the stimulus is red or where it falls within the fovea. At the same time the effect upon the more obtuse cones of the same area has been masked by the greater effect on the more sensitive visual purple. The bleaching of this substance, however, is not instantaneous but follows a temporal course of a rise to a maximum, where it remains for a few seconds, before it is overcome by the regenerative effect of the subsequent darkness. The blurring or spreading out of the image is probably correlated with the diffusion of the chemical by-products of the bleaching as the regeneration begins to set in. This explanation is, of course, tentative, but it gives a likely picture in terms of the present accepted visual theory of the physiological elements.

Relation to Earlier Observations. The after-image we have been observing without doubt has been observed in previous investigations, but though we assumed that we should find a description of it and its essential conditions in the literature, apparently no experimenter takes note of it as a type of after-image distinct from the ordinary positive.

G. E. Müller, who gives us the most recent and detailed summary in the field, lists nine possible stages in the after-effect of a brief light stimulus.

> "Der Nachbildverlauf bei kurz dauernden Lichtreizen.
>
> "I. Die verschiedenen Nachbildphasen. Der durch einen kurz dauernden Lichtreiz bewirkte Bildverlauf lässt in der

> Regel eine Mehrschlägigkeit erkennen. Ist dieselbe voll entwickelt, so lassen sich nach den überlichen Darstellungsweisen die nachstehenden, aufeinander folgenden Phasen unterscheiden.
>
> "1. Das Primärbild.
>
> "2. Ein sehr kurzes, dunkles Intervall.
>
> "3. Das Heringsche Nachbild, ein helles (positives), wenn auch dem Primärbild an Helligkeit nachstehendes Bild, das bei farbigem Reizlicht diesem gleichgefärbt ist.
>
> "4. Ein dunkles Intervall, das bedeutent länger dauert als das als zweite Phase angeführte dunkle Intervall.
>
> "5. Das Purkinjesche Nachbild, ein helles, aber dem Heringschen Nachbilde an Helligkeit nachstehendes Bild, das bei farbigem Reizlichte meistens komplementär gefärbt ist.
>
> "6. Ein dunkles Intervall, das noch länger dauert, aber weniger dunkel is als das an vierter Stelle angeführte dunkle Intervall.
>
> "7. Ein im allgemeinen lang andauerndes positives, aber nur sehr wenig helles Nachbild, das wir nach dem Vorgange von Dittler und Eisenmeier kurz das Hesssche Nachbild nennen wollen. Bei farbigem Reizlichte besitzt es im allgemeinen die gleiche oder eine ähnliche Farbe wie dieses, wenn auch nur in sehr geringer Ausgeprägtheit.
>
> "8. Ein negatives, bei farbigen Lichtreize komplementär gefärbtes Nachbild.
>
> "9. Nach diesem wird in seltenen Fällen noch ein viertes positives Nachbild beobachtet.
>
> "Es hängt von den Versuchsbedingungen ab, wie viele und welch von diesen 9 Phasen zur Beobachtung kommen. Am häufigsten erörtert und namentlich von v. Hess mittles der Bewegungsmethode sehr eingehend untersucht wurde der Fall der Dreischlägigkeit, in dem nur 3 helle Bilder, nämlich das Primärbild, das Purkinjesche und das Hesssche Nachbild auftreten (4, pp. 177-178)."

The stages reported by some investigators differ from Müller's, particularly in the number of the images and in the colors in the odd-numbered stages. Stage 3, the "Hering after-image" or the "secondary image" (Parsons), which corresponds temporally with the image we have been examining, has been reported as anything from colorless to a strong positive. Parsons calls it "the ordinary positive after-image," but he notes that it is more complex than this designation would indicate.

> "With an object light of low intensity it is a pure scotopic reaction, as shown by its grey quality and its absence with a pure red object light. With a brighter object light it is a combined photopic and scotopic reaction, the photopic preponderating with strong lights, as shown by the colour and saturation of the secondary image" (5, pp. 90-91).

This is in agreement with our own observations, but it is surprising that no one seems to have been interested in controlling conditions in such a way that the scotopic portion of the image could be set off by itself. For

TABLE 2

	Swindle	McDougall	Dimmick
Conditions	Positive after-image of long duration	Secondary image	Scotopic image
	Dark-adaptation (5-10 min.)	Dark-adaptation (?)	Dark-adaptation (5-10 min.)
	Fixation		Indirect fixation
	Stimulus—bright illumination, several successive exposures, .5 sec. each; after 6 exposures pause—5 sec.—followed by .5-sec. stimulus	Stimulus fairly intense—duration .05 sec.	Stimulus fairly intense—duration 1 sec.
After-images	Positive, i.e., same color as stimulus	Faint gray, tinged with color of stimulus	Light bluish grey, no color from stimulus either positive or negative
	Duration up to 54 sec.	Duration approx. 10 sec.	Duration 10 sec. or longer
		Absent with red light	Red appears black; absent in fovea
	"Very good," "very real."		Perceptually complete
		Rod and cone effect	Cone effect

purposes of ready comparisons we have listed in Table 2 in parallel columns the various aspects of the images described in two other investigations and our own.

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PREFERENCES FOR RECTANGLES

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Since the times of Zeising's (4) speculations and Fechner's (1) experiments there have been occasional treatises issued on the problem of the golden section, the rectangle whose sides are to each other as 3 to 5 (better still, 21 to 34). Several of the researchers have claimed that while this particular form is well liked it is not the most preferred. Quite recently Miss Gordon (2) has again found the golden section to be the most preferred of the ten rectangles she employed as stimuli. The Gordon article has stimulated the present writer to publish data which he has been collecting for some time, as they seem pertinent to this problem.

For the past five years the writer has given rectangle preference tests to his laboratory classes. On these occasions the golden section has always received first or second place in the preference scale. During the winter quarter of 1932 the experiment was somewhat expanded and outside groups were tested. The added subjects were all drawn from the elementary course in psychology. The 1932 experiment had two parts. In the first section, nine rectangular cardboards with identical areas (1162.81 sq. cms.) were exposed on an easel to 22 subjects. The paired comparisons method was used. The rectangles[1] were presented in two positions, *upright* (with the narrow side uppermost) and *side* (with the wide side uppermost). By virtue of this technique there were 17 stimuli (one of the forms, the square, is identical in the two positions). The exposure time was approximately 10 seconds for each stimulus pair. Thirty seconds elapsed between pairs. The cardboards were neutral gray; the easel was dark gray.

In the experiments of past years the upright positions alone had been employed. It is to be presumed that Miss Gordon also presented her stimuli in the upright position. The rank order found in the present experiment (upright position) correlated quite well with those of former years (from .75 to .93).[2] However, there was a value of only .35 between the present rank order and Gordon's. The latter experimenter employed a ranking method, a fact which may partially account for the low coefficient. A rho

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[1] The dimensions of the stimuli are given in the tables.

[2] All values in terms of rho.

TABLE 1

Position	Horizontal	Vertical	Section 1 Sigma Unit Value	Section 2 Sigma Unit Value
Side	34	21	+.08	+.18
Upright	21	34	+.52	—.02
	1	1	—.19	+.05
Side	6	5	—.36	+.07
Upright	5	6	+.21	+.15
Upright	4	5	+.24	+.02
Side	5	4	—.20	—.07
Side	4	3	—.23	+.15
Upright	3	4	+.41	+.13
Side	2	1	—.20	—.02
Upright	1	2	+.16	—.25
Side	3	2	+.47	+.20
Upright	2	3	—.02	+.05
Side	5	2	—.57	—.23
Upright	2	5	—.12	—.71
Upright	29	30	—.09	+.05
Side	30	29	—.30	+.13

of .44 obtained between the rank orders of the present writer's *upright* cardboards and their corresponding *side* partners.

By means of the Guilford (3) technique the preferences as found in the present experiment were arranged in terms of sigma units. They are given in the Table 1 under *Section 1.* From this table it can be seen that the upright position of the golden section received first place in the preference series of Section 1.

In the second section of the present experiment the procedure was changed in certain respects. The easel was replaced by a tachistoscope. Instead of the cardboards, there were white exposure cards with black rectangles painted on them. Each rectangle area was only 14.44 sq. cms.; each card background occupied 223.38 sq. cms. As the stimuli were quite small the 61 subjects were tested in two groups to facilitate vision. Care was taken that the well-illuminated cards were not viewed at an angle. In both sections of the experiment the cooperation of the subjects appeared good, even though the number of stimulus pairs was extremely large. The values in terms of sigma units are given in the table. A rho of .20 was found to exist between this series and that of Section 1. For the upright exposures the value was .15; for the side it was .55. The rho between the upright and side positions of Section 2 was .47.

The following conclusions seem warranted.

1. The relative positions of unfilled rectangles in preference series remain consistently similar provided the stimuli and methods of their presen-

tation are roughly identical, and the subjects are of approximately the same social status.

2. The exposure position of the rectangle, i.e., whether the longer dimension is in the vertical or the horizontal, also has considerable effect on the preference value.
3. Changes in the stimuli, i.e., changes in the size and color of the rectangles, greatly affect the preference value.
4. Similarly, the preference status of the golden section is a function of the size and color of the rectangle, the position in which it is presented, and perhaps of other untested variables. In conditions somewhat approximating Fechner's experimental situation it has a high relative rating.

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A PARADOXICAL COLOR EFFECT

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When the accompanying figure is held about two feet from the eye and looked at under low white-light illumination, the discs appear as if colored. A suitable illumination can be conveniently obtained by viewing the figure in room light through a pin-hole. The colors are usually of low saturation but are quite definite. Most of the persons I have tested have reported a rose or a purplish red, a yellow, and a green or blue-green. Others have reported only a blue and yellow. Each disc takes on a single color and retains it as long as the fixation is not changed. Adjacent discs may be and usually are, of different colors.

An explanation suggests itself in terms of a functional-element theory of color vision. At the necessary low illumination the number of functional cones within the retinal image of a single disc cannot be very large. If the number is sufficiently low, a proportionate distribution of elementary types is improbable or even impossible. But a disproportionate activity is, by hypothesis, the characteristic effect of stimulation by colored light.

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It should not be difficult to test this explanation. A quantitative investigation of the optimal illumination and the optimal visual angle subtended by a disc is being planned. The distribution of the several hues and possible differences in saturation are also of obvious significance in color vision theory.

So far as I am aware, no comparable effect has been reported.

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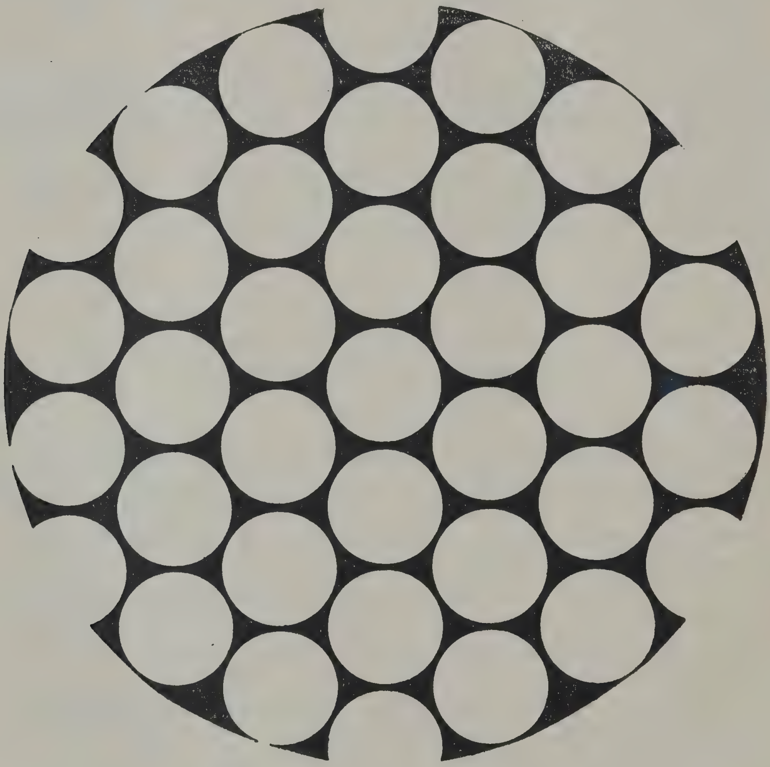

APPARATUS

A TIME MARKER FOR 120 CYCLES

HAROLD SCHLOSBERG

For several years the writer has been using a tuned time marker that operates on 60-cycle A.C., and gives a smooth sigmoid curve of 120 cycles per second. The marker is simple, easily adjusted, accurate, quiet, and is durable and powerful enough to be used regularly by untrained students. The vibrating element (*1*) is a 5-inch piece of piano wire (B & S No. 18), held rigidly at one end, and fitted with a light celluloid tip at the other. The wire is tuned with an adjustable bridge (*B*), and the vibrations maintained by a small coil (*C*) near the free end.

The elements are mounted on a square brass rod (*2*), and consist of a coil (*C*), bridge (*B*), and wire holder (*A*). The coil is one taken from a pair of head phones rewound with 40 feet of No. 28 enamelled wire. This procedure is much simpler than constructing a whole new coil, and insures a small coil of low lag. The coil is mounted at one end of the square rod, which may be ground down if a more compact marker is desired.

At the opposite end of the square rod is mounted the holder (*A*) for the fixed end of the vibrating wire. It consists of an inverted 'L' of strip brass with a groove cut in the top. The wire (*L*) is held in the groove by a cap and screw. The top of the holder should be 1/4 inch higher than the top of the coil and sloping down forward to give the wire sufficient tension to hold it against the bridge.

The bridge (*B*) slides on the square rod between support and coil. It consists of a square sleeve, with set screw (*4*), bearing a vertical screw (*5*) (8-32) and lock nut. The end of this screw is ground into a hollow V-knife edge, which supports the vibrating wire. By sliding the whole bridge along the square rod, the projecting wire may be tuned for frequency. The vibration amplitude may be varied by raising or lowering the screw a half turn, thus varying the distance between coil and wire.

For convenience in mounting the marker, the square rod is extended with a piece of 3/8-inch brass tube (*6*). The wires from the coil are encased in tubing ("spaghetti"), slipped through the brass tube, and brought out the end for attachment to the source of current. The marker may be operated from a 10-volt bell-ringing transformer, using a 50-ohm rheostat, if desired. The writer finds it simpler to make them up into well-insulated units, consisting of a marker in series with a 50-watt bulb and a plug for insertion into a lighting circuit. If a half ampere is passed through the coil, it will not overheat, and will furnish abundant power. The marker must be returned whenever a new celluloid tip is added. This may best be

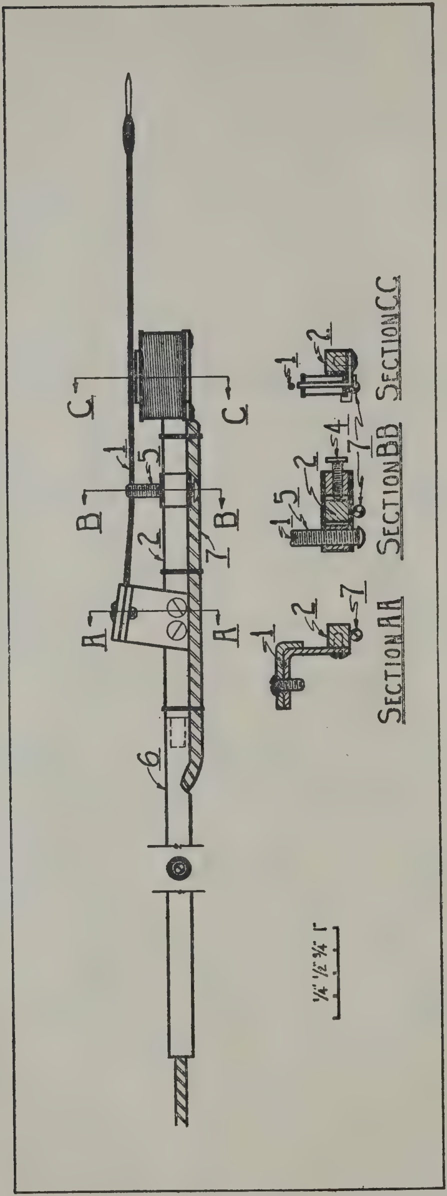
A
A
B
B
C
C
Section AA
Section BB
Section CC
1/4" 1/2" 3/4" 1"

FIGURE 1

done, of course, with the current turned on. Use a small tip and little wax. For use at 120 cycles, the wire should project about 2 1/2 inches beyond the bridge. If regulated alternating current is not available, the marker may be operated through a 100-cycle electrically maintained fork. This will not be as accurate as regulated A.C. If a slower vibration is desired, the length of the vibrating wire may be doubled. The marker can then be operated through a rectifier at 60 cycles.

In adjusting the marker, there is the possibility of tuning it to some multiple of the actuating frequency. This may be checked by comparing its tracing with that of a fork or a pendulum of known period. It must be remembered that there will be either a large error (100%) or none at all. Unlike a fork, this marker is roughly tuned, and follows the actuating pulses accurately. If these pulses are obtained from a regulated power line (e.g., one that can be used to run an electric clock) the marker will probably be more accurate than any fork used to check it, except for the possibility mentioned above.

The marker is quite compact, measuring less than 1 1/2 inches in height. This, coupled with the fact that the stylus is at the extreme top of the marker, makes it possible to get it close to other markers. It may be inverted, if desired. The pressure exerted on the drum by the marker is not important, but the amplitude of vibration must be kept down to 1/4 inch, to prevent "chattering." If properly adjusted, the marker makes a barely audible hum when held firmly in a clamp. This may be practically eliminated by slipping a piece of rubber between the clamp and marker. It will then give a reliable time line, with freedom from distracting noise, for as long a period as desired, without further attention.

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A NEW MAZE TECHNIQUE

Wilton P. Chase

The description which follows is of a maze which I shall call a light maze which was constructed for the purpose of limiting the kinaesthetic factors which are present in maze learning of the ordinary variety. The developmental and comparative study of the different types of mazes has been made by Knotts and Miles (2), Miles (3, 4), Barker (1), and Perkins (5). No further review need be made here.

The light maze is illustrated in Figure 1. It is simply a box with the top covered by frosted glass and one long open side. On the frosted glass are glued 13 rows of 1-inch black squares, 11 squares to the row. The squares are glued 1/4 inch apart on all sides. The box is placed on ball casters, in order to permit it to be moved in any direction.

A round point of light about 1/4 inch in diameter is projected upon the ground glass alleys from a light which is placed inside the box. Blind alleys may be blocked off by placing small squares of heavy black paper across the alleys on the inside of the glass top.

A screen is erected in front of the maze behind which the subject sits. He looks through an eye-piece into two tubes. The tube for the left eye is blocked off entirely. The tube for the right eye is adapted with an opening 5 mm. in diameter directly in front of the eye and an opening 15 mm. in diameter at the end of the tube. This permits the subject to see but one corner at a time. The light is centered in the middle of the lower opening by means of double threads placed 1/4 inch apart, crossing at right angles in the center.

The apparatus is best used in a dark room. The subject directs the experimenter as to how he wants the light to move. The directions of north, east, south, and west are used very handily. At each corner he has three choices. If he chooses a blind alley, the light will disappear about half way down the alley as the box is moved so that the black square across the alley will cover the light. If a right choice is made, which may be the first, second, or third direction chosen, the light will be seen by the subject to move to the next corner, where he again makes a choice.

Thus, we have an "ideational" situation, employing the counting or verbal method of maze learning. The purpose of the light and the alleys is to reproduce a comparable situation to a stylus maze. They indicate the subject's wrong and right choices, and help him to keep in mind what he is doing, namely, to make the right choice at each corner and to count the number of moves in each direction as he progresses from the beginning to the end of the maze.

If the subject is thoroughly interested, backward moves are eliminated, because he will remember from which direction he has approached a corner. If he forgets, he can be told that he is going backwards, if the experimenter so desires.

An electric counter is necessary for the experimenter to count the errors. This can be operated from a foot switch as the box is being operated with both hands.

The kinaesthetic factors are limited because the subject has no chance to use hands or arms and the head is held steady by looking through the tube at the light. The arrangement of the tube through which he looks is devised to control eye-movements.

In order to have a maze which involves kinaesthesis and which presents a comparable situation with the light maze, the following stylus maze was constructed.

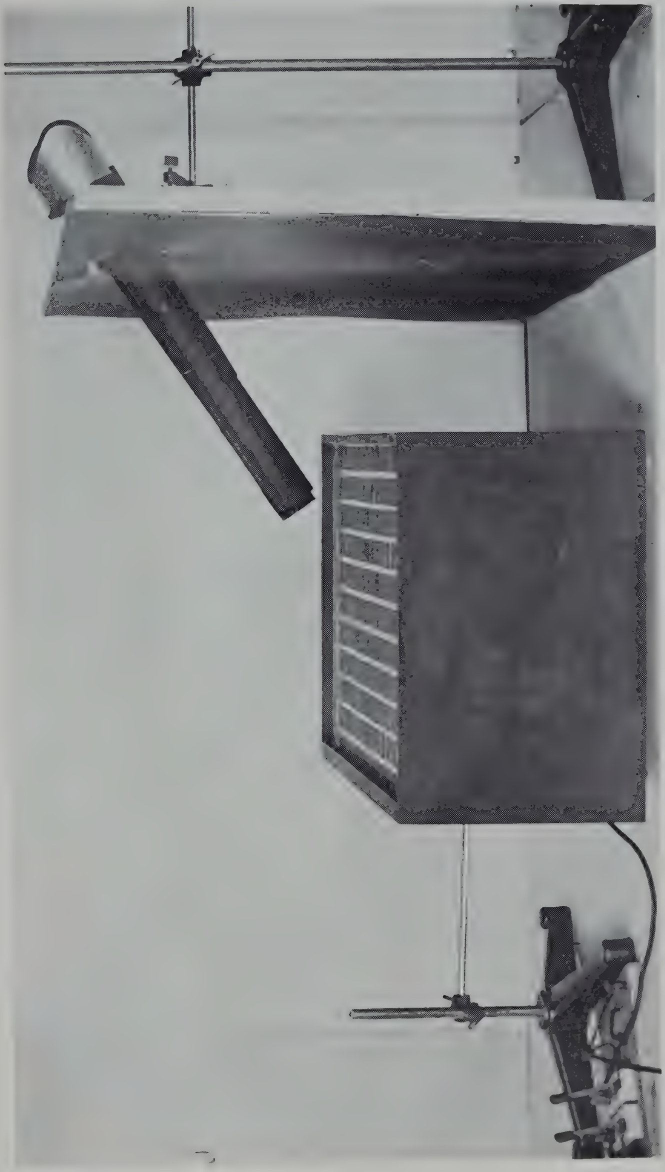

FIGURE 1

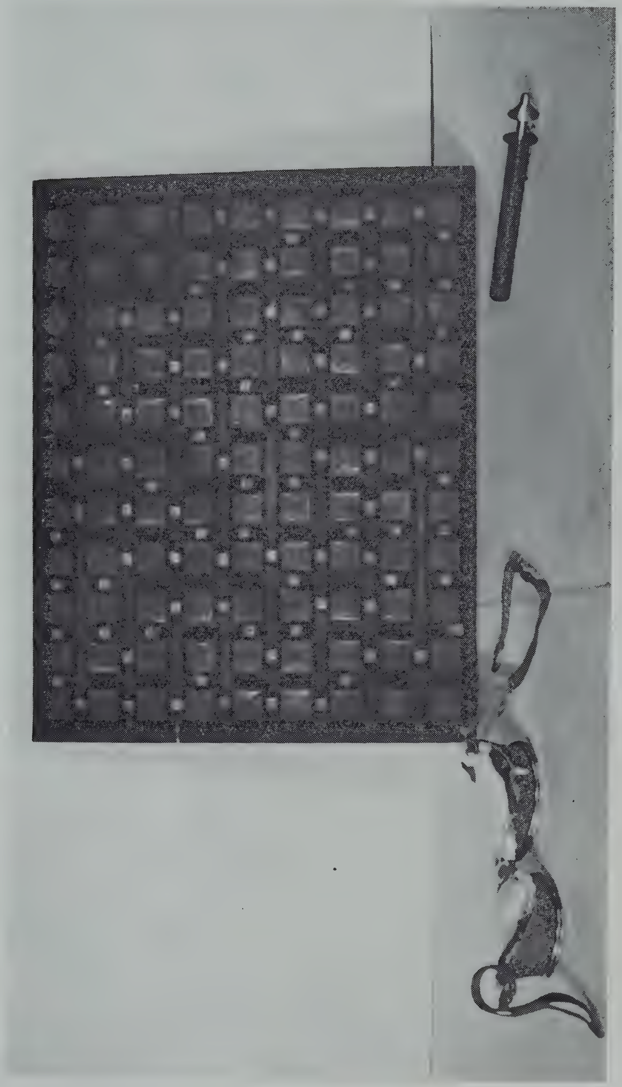

FIGURE 2

The stylus maze is illustrated in Figure 2. It is constructed by glueing 11 rows of 1-inch matched hardwood squares, with bevelled edges, 9 squares to a row. The squares are placed 1/4 inch apart on all sides. Small 1/4-inch squares of hard rubber can be used to block off blind alleys. The stylus has a tapering point, with a bevelled flange, so as to allow it to fit on both edges of the alley. The stylus fits easily into the grooves, and when it comes to a corner, it drops into it sufficiently to be perceptible to the subject. This permits elimination of backward movements in the same manner as in the light maze. As in the light maze the subject has three choices at each corner. Here, also, the counting method of maze learning is obtained, as the subject can easily count the number of squares which he traverses in each direction from the beginning to the end of the maze.

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